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AMERICAN SOCIETY

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CIVIL ENGINEERS.

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AMERICAN SOCIETY OF CIVIL ENGINEERS.
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TRANSACTIONS.

Paper No. 956.

DEFLECTIONS OF BEAMS WITH VARIABLE MOMENTS OF INERTIA.

By C. W. Hudson, M. Am. Soc. C. E.

With Discussion by Messrs. Irving P. Church, Mansfield Merriman, C. H. Lindenberger and C. W. Hudson.

The determination of the deflections for the special types of open-webbed, elastic frameworks often met in the practice of the bridge engineer has received much attention, and very exact solutions are made for these problems by both graphical and analytical methods.

The determination of the deflections for solid-webbed girders, which is of equal theoretical interest and often of great practical importance, however, has been confined chiefly to girders with a constant moment of inertia, and such determinations have usually been made by means of the equation of the elastic curve. It is the purpose of this paper to give a more general method for determining the deflections of solid-webbed girders under flexure, and to illustrate its application to the forms of girders most frequently met in practical bridge construction. The plate-girder drawbridge, owing to its frequent use, is one of the most important structures with which the bridge engineer has to deal. Application of the general formula to this case has been made, with the object of giving a simple method of making a better design for the girders and end-lifting machinery for these bridges.

* Presented at the Meeting of May 6th, 1903.

The writer believes that the determination of the deflection of girders with solid webs is effected best by means of the method of work. The general formula is derived, it will be noticed, in a manner very similar to that commonly used in obtaining the general formula for the deflections of articulate structures.

Let us suppose a beam of any shape in equilibrium under the action of a given loading, and that the following derivation is made under the usual conditions imposed in the development of the fundamental formulas for flexure, and under the supposition that, where the forces producing bending have components in the direction of the length of the beam, the effect of such components in producing distortion may be neglected.

Let $M =$ the bending moment at any section of the beam due to the given loading;

$S =$ the unit stress due to flexure on any fiber of the cross-section, the fiber being distant y from the neutral axis, and

$I =$ the moment of inertia of the cross-section for which the other quantities are taken.

Then, from the fundamental relation for flexure, we have $S = \frac{My}{I}$.

Under the action of flexure, the fibers on one side of the neutral axis of a beam are lengthened, and on the other side they are shortened.

Let dx (an infinitesimal) be a portion of the length of the beam, and λ be the change in length of any fiber distant y from the neutral axis, due to flexure.

From Hooke's Law, we have $\lambda = \frac{SL}{E}$, in general, and for a fiber of a length dx, $\lambda = \frac{S\,dx}{E}$, in which E is the coefficient of elasticity of the material.

Since $S = \frac{My}{I}$,

$$\lambda = \frac{My}{EI}\,dx.$$

Now, simultaneously with the given loading, suppose a force of unity to be applied to the beam at any point in any desired direction. Let m be the bending moment at any section of the beam thereby produced. The stress on any fiber (the same fiber for which λ is taken) of

any section due to the assumed load of unity $= \frac{m\,y}{I}$, and the work done by this stress, assuming it to be gradually produced, acting through the distance λ, is equal to $\frac{m\,y}{2\,I} \times \lambda = \frac{M\,y}{2\,I} \times \frac{m\,y}{E\,I}\,dx = \frac{M\,m\,y^2\,dx}{2\,E\,I^2}$.

Letting $\int dA$ represent the area of the entire cross-section, the work done on an infinitesimal portion of the length of the beam $= \frac{M\,m}{2\,E\,I^2}\left(\int y^2\,dA\right)dx$, and since $\int y^2\,dA = I$, this work $= \frac{M\,m\,dx}{2\,E\,I}$, and for the internal work throughout the length of the beam due to flexure we have:

$$W = \int_o^l \frac{M\,m\,dx}{2\,E\,I} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

Again, let Δ be the deflection, due to the given loading, of the point at which the force of unity is applied, measured along the line of action of the force, unity, then the work done by the force of unity gradually applied is:

$$W = \frac{1}{2} \times \Delta = \frac{\Delta}{2} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2)$$

Making the total work of the internal stresses due to flexure (1) equal to the total work of the external forces (2), we have:

$$\frac{\Delta}{2} = \int_o^l \frac{M\,m\,dx}{2\,E\,I}$$

$$\Delta = \int_o^l \frac{M\,m\,dx}{E\,I}\,* \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

which is the general value for the deflection of any point in a beam.

As an illustration of the general application of this formula, suppose we find the horizontal deflection of the upper corner of a simple I-beam span, loaded at the center with the weight P. Fig. 1 will make the problem more clear.

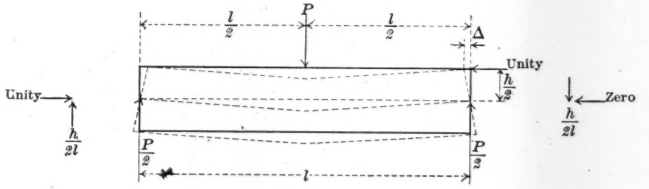

FIG. 1.

*This formula was first derived by Professor Fraenkel.

Taking each half of the beam separately, and taking the ends of the neutral axis of the beam as the origin of x, we have:

$$\Delta = \int_o^l \frac{M m\, dx}{EI} = \int_o^{\frac{l}{2}} \frac{M m\, dx}{EI} \text{ for the left half}$$

$$+ \int_o^{\frac{l}{2}} \frac{M m\, dx}{EI} \text{ for the right half,}$$

in which,

$$M = + \frac{P}{2} \times x, \text{ for both right and left halves of the beam,}$$

$$m = + \frac{h}{2l} \times x, \text{ for the left half of the beam,}$$

$$\text{and } m = - \frac{h}{2l} \times x + \frac{h}{2}, \text{ for the right half of the beam.}$$

Substituting these values for M and m in the general equation, we have:

$$\Delta = \int_o^{\frac{l}{2}} \frac{P h x^2 dx}{4 l EI} - \int_o^{\frac{l}{2}} \frac{P h x^2 dx}{4 l EI} + \int_o^{\frac{l}{2}} \frac{P h x\, dx}{4 EI}$$

$$= \int_o^{\frac{l}{2}} \frac{P h x\, dx}{4 EI} = \frac{P h l^2}{32 EI}.$$

If m make $P = 29\,000$, $l = 360$ ins. and $I = 1\,140$ (inches ^{4th}), this being the value for the moment of inertia of a 20-in. I-beam (192 lbs per yard), then:

$$\Delta = \frac{29\,000 \times 20 \times (360)^2}{32 \times 29\,000\,000 \times 1\,140} = \frac{1}{14} \text{ in., nearly.}$$

In the problems of practice it is usually the vertical deflection of some point in a horizontal beam under vertical loading that is desired. The following five problems are deemed sufficient to show the application of the general formula to finding such deflections. The girders in the following problems have four values for the moment of inertia; the resulting expressions for the deflections, however, are of such form that expressions for the value of the deflection for a girder with a greater or less number of values of the moment of inertia can be written by inspection. For girders with inclined flanges, values of the moment of inertia can be determined at stated intervals, and these values considered constant for the intervals taken.

Problem No. 1.—Find the vertical deflection at the center of a plate-girder span with three cover-plates, the girder being loaded uniformly

throughout its length, l, with a vertical load of w pounds per unit of length. Fig. 2 will make the problem more clear.

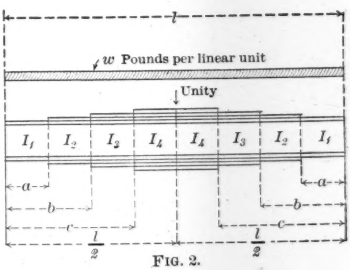

FIG. 2.

As the moment of inertia of the girder has four values, the second part of the general equation,

$$\Delta = \int_o^l \frac{M m \, d x}{E I} = 2 \int_o^{\frac{l}{2}} \frac{M m \, d x}{E I},$$

will have four parts:

$$\Delta = \frac{w}{2E} \int_o^a \frac{(l x^2 - x^3) \, d x}{I_1} + \frac{w}{2E} \int_a^b \frac{(l x^2 - x^3) \, d x}{I_2} +$$

$$+ \frac{w}{2E} \int_b^c \frac{(l x^2 - x^3) \, d x}{I_3} + \frac{w}{2E} \int_c^{\frac{l}{2}} \frac{(l x^2 - x^3) \, d x}{I_4}$$

$$= \frac{w}{2E} \left\{ \frac{l a^3}{3 I_1} - \frac{a^4}{4 I_1} + l \frac{(b^3 - a^3)}{3 I_2} - \frac{b^4 - a^4}{4 I_2} + l \frac{(c^3 - b^3)}{3 I_3} \right.$$

$$\left. - \frac{c^4 - b^4}{4 I_3} + \frac{l (l^3 - 8 c^3)}{24 I_4} - \frac{l^4 - 16 c^4}{64 I_4} \right\}$$

$$= \frac{5}{384} \frac{w l^4}{E I_4} + \frac{w l}{6 E} \left(\frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4} \right)$$

$$- \frac{w}{8 E} \left(\frac{a^4}{I_1} - \frac{a^4}{I_2} + \frac{b^4}{I_2} - \frac{b^4}{I_3} + \frac{c^4}{I_4} - \frac{c^4}{I_4} \right)$$

which is the expression for the value of the desired deflection.

If $I_1 = I_2 = I_3 = I_4$, that is, if the moment of inertia of the beam be constant, the expression becomes:

$$\Delta = \frac{5}{384} \frac{w l^4}{E I},$$

which is the well-known formula for the deflection at the center of a simple beam of constant moment of inertia, under uniform loading.

Problem No. 2.—Find the vertical deflection at the center of a plate-girder span with three cover-plates, due to a vertical load, P, at any point in the span.

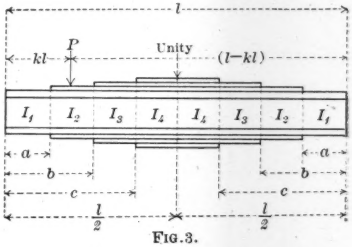

FIG. 3.

The load, P, will be placed on the first cover-plate, and the

expression found for the value of the deflection. The expression for the deflection for any desired position of P can then be written by inspection.

$$\Delta = \int_o^l \frac{M m \, dx}{E I}$$

$$= \int_o^a \frac{P(1-k)x^2\,dx}{2EI_1} + \int_a^{kl} \frac{P(1-k)x^2\,dx}{2EI_2}$$

$$+ \int_k^b \frac{Pk(lx-x^2)\,dx}{2EI_2} + \int_b^c \frac{Pk(lx-x^2)\,dx}{2EI_3}$$

$$+ \int_c^{\frac{l}{2}} \frac{Pk(lx-x^2)\,dx}{2EI_4} + \int_o^a \frac{Pkx^2\,dx}{2EI_1} + \int_a^b \frac{Pkx^2\,dx}{2EI_2}$$

$$+ \int_b^c \frac{Pkx^2\,dx}{2EI_3} + \int_c^{\frac{l}{2}} \frac{Pkx^2\,dx}{2EI_4}$$

$$= \frac{P(1-k)}{6E}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{k^3l^3}{I_2}\right)$$

$$+ \frac{Pk}{4E}\left(\frac{lb^2}{I_2} - \frac{lb^2}{I_3} + \frac{lc^2}{I_3} - \frac{lc^2}{I_4} + \frac{l^3}{4I_4} - \frac{l^3k^2}{I_2}\right)$$

$$- \frac{Pk}{6E}\left(\frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4} + \frac{l^3}{8I_4} - \frac{k^3l^3}{I_2}\right)$$

$$+ \frac{Pk}{6E}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4} + \frac{l^3}{8I_4}\right)$$

$$= \frac{P}{6E}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2}\right) - \frac{Pk^3l^3}{12EI_2}$$

$$+ \frac{Pkl}{4E}\left(\frac{b^2}{I_2} - \frac{b^2}{I_3} + \frac{c^2}{I_3} - \frac{c^2}{I_4}\right) + \frac{Pkl^3}{16EI_4},$$

which is the expression for the value of the desired deflection.

For a load, P, on the second cover-plate, by inspection, can be written:

$$\Delta = \frac{P}{6E}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3}\right) - \frac{Pk^3l^3}{12EI_3}$$

$$+ \frac{Pkl}{4E}\left(\frac{c^2}{I_3} - \frac{c^2}{I_4}\right) + \frac{Pkl^3}{16EI_4}.$$

If we make $I_1 = I_2 = I_3 = I_4$, in either of the above values for Δ, we have:

$$\Delta = \frac{Pkl^3}{48EI}(3 - 4k^2),$$

and if $k = \frac{1}{2}$, that is, for P in the center of the girder,

$$\Delta = \frac{Pl^3}{48EI}.$$

Problem No. 3.—Find the vertical deflection at the loaded point of a plate-girder span with three cover-plates, due to a vertical load, P, at any point in the span.

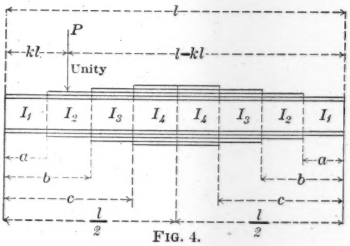

FIG. 4.

P will be placed on the first cover-plate, as in Problem No. 2.

$$\Delta = \int_o^l \frac{M\,m\,d\,x}{E\,I}$$

$$= \int_o^a \frac{P\,(1-k)^2\,x^2\,d\,x}{E\,I_1} + \int_a^{k\,l} \frac{P\,(1-k)^2\,x^2\,d\,x}{E\,I_2}$$

$$+ \int_{k\,l}^b \frac{P\,k^2\,(l-x)^2\,d\,x}{E\,I_2} + \int_b^c \frac{P\,k^2\,(l-x)^2\,d\,x}{E\,I_3}$$

$$+ \int_c^{\frac{l}{2}} \frac{P\,k^2\,(l-x)^2\,d\,x}{E\,I_4} + \int_o^a \frac{P\,k^2\,x^2\,d\,x}{E\,I_1}$$

$$+ \int_a^b \frac{P\,k^2\,x^2\,d\,x}{E\,I_2} + \int_b^c \frac{P\,k^2\,x^2\,d\,x}{E\,I_3} + \int_c^{\frac{l}{2}} \frac{P\,k^2\,x^2\,d\,x}{E\,I_4}$$

$$= \frac{P\,(1-k)^2}{3\,E}\left(\frac{a^3}{I_1} + \frac{k^3\,l^3}{I_2} - \frac{a^3}{I_2}\right)$$

$$+ \frac{P\,k^2\,l^2}{E}\left(\frac{b}{I_2} - \frac{k\,l}{I_2} + \frac{c}{I_3} - \frac{b}{I_3} + \frac{l}{2\,I_4} - \frac{c}{I_4}\right)$$

$$- \frac{P\,k^2\,l}{E}\left(\frac{b^2}{I_2} - \frac{k^2\,l^2}{I_2} + \frac{c^2}{I_3} - \frac{b^2}{I_3} + \frac{l^2}{4\,I_4} - \frac{c^2}{I_4}\right)$$

$$+ \frac{P\,k^2}{3\,E}\left(\frac{b^3}{I_2} - \frac{k^3\,l^3}{I_2} + \frac{c^3}{I_3} - \frac{b^3}{I_3} + \frac{l^3}{8\,I_4} - \frac{c^3}{I_4}\right)$$

$$+ \frac{P\,k^2}{3\,E}\left(\frac{a^3}{I_1} + \frac{b^3}{I_2} - \frac{a^3}{I_2} + \frac{c^3}{I_3} - \frac{b^3}{I_3} + \frac{l^3}{8\,I_4} - \frac{c^2}{I_4}\right)$$

$$= \frac{P\,l^3\,k^2}{3\,E}\left(\frac{1}{I_4} - \frac{2\,k}{I_2} + \frac{k^2}{I_2}\right) + \frac{P\,(1-2\,k)}{3\,E}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2}\right)$$

$$+ \frac{P\,k^2\,l^2}{E}\left(\frac{b}{I_2} - \frac{b}{I_3} + \frac{c}{I_3} - \frac{c}{I_4}\right)$$

$$- \frac{P\,k^2\,l}{E}\left(\frac{b^2}{I_2} - \frac{b^2}{I_3} + \frac{c^2}{I_3} - \frac{c}{I_4}\right)$$

$$+ \frac{2\,P\,k^2}{3\,E}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_4} - \frac{c^3}{I_4}\right),$$

which is the expression for the value of the desired deflection.

If we make $I_1 = I_2 = I_3 = I_4$, then

$= \frac{P\,l^3\,k^2\,(1-k)^2}{3\,E}$, and, in addition, if we make $k = \frac{1}{2}$, then

$$\Delta = \frac{P\,l^3}{48\,E\,I}.$$

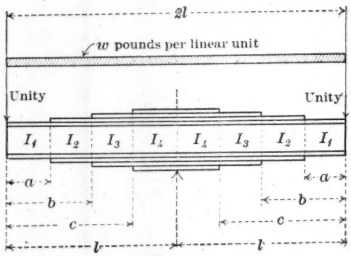

FIG. 5.

Problem No. 4.—Find the vertical deflections of the ends of a plate-girder span with three cover-plates, supported at the center, only, and loaded with a uniform vertical load, w, per unit of length.

$$\Delta = \int_o^l \frac{M\,m\,d\,x}{E\,I}$$

$$= \int_o^a \frac{w\,x^3\,d\,x}{2\,E\,I_1} + \int_a^b \frac{w\,x^3\,d\,x}{2\,E\,I_2} + \int_b^c \frac{w\,x^3\,d\,x}{2\,E\,I_3} + \int_c^l \frac{w\,x^3\,d\,x}{2\,E\,I_4}$$

$$= \frac{w}{8\,E}\left(\frac{a^4}{I_1} + \frac{b^4}{I_2} - \frac{a^4}{I_2} + \frac{c^4}{I_3} - \frac{b^4}{I_3} + \frac{l^4}{I_4} - \frac{c^4}{I_4}\right)$$

$$= \frac{w\,l^4}{8\,E\,I_4} + \frac{w}{8\,E}\left(\frac{a^4}{I_1} - \frac{a^4}{I_2} + \frac{b^4}{I_2} - \frac{b^4}{I_3} + \frac{c^4}{I_3} - \frac{c^4}{I_4}\right)$$

which is the expression for the value of the desired deflection.

If $I_1 = I_2 = I_3 = I_4$, then,

$$\Delta = \frac{w\,l^4}{8\,E\,I}.$$

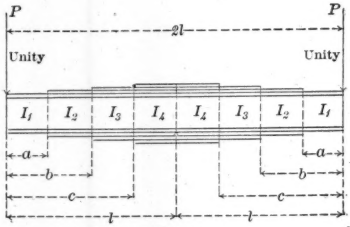

FIG. 6.

Problem No. 5.—Find the vertical deflection of the ends of a plate-girder span with three cover-plates, supported at the center, only, and loaded at each end with a load, P.

$$\Delta = \int_o^l \frac{M\,m\,d\,x}{E\,I}$$

$$= \int_o^a \frac{P\,x^2\,d\,x}{E\,I_1} + \int_a^b \frac{P\,x^2\,d\,x}{E\,I_2} + \int_b^c \frac{P\,x^2\,d\,x}{E\,I_3} + \int_c^l \frac{P\,x^2\,d\,x}{E\,I_4}$$

$$= \frac{P}{3\,E}\left(\frac{a^3}{I_1} + \frac{b^3}{I_2} - \frac{a^3}{I_2} + \frac{c^3}{I_3} - \frac{b^3}{I_3} + \frac{l^3}{I_4} - \frac{c^3}{I_4}\right)$$

$$= \frac{P\,l^3}{3\,E\,I_4} + \frac{P}{3\,E}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4}\right)$$

which is the expression for the value of the desired deflection.

If $I_1 = I_2 = I_3 = I_4$, then,

$$\Delta = \frac{P\,l^3}{3\,EI}.$$

By substituting for Δ, in the above expression, the distance it is desired to raise the end of a plate-girder draw, and solving for P, the uplift necessary to raise the end the desired amount is obtained.

In applying the results of the preceding problems, a deduction of 2 or 3 ft. from the over-all lengths of the cover-plates should be made, for the reason that enough rivets to fully develop the value of a cover-plate to resist stress are not generally contained in a less distance than the first 2 or 3 ft. at the end of the cover-plate.

By the aid of the results of Problems Nos. 4 and 5, the reactions for center-bearing plate-girder draw spans, with equal arms under uniform loading, can be obtained.

Let $R_1 = R_3$ be the reactions at the ends of a two-span plate-girder with three cover-plates. From the result of Problem No. 5, we have, for the vertical motion, Δ, due to a force, $R_1 = R_3$, at the ends, the following:

$$\Delta = \frac{R_1\,l^3}{3\,E\,I_4} + \frac{R_1}{3\,E}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4}\right)$$

If, at the same time, we make the value for Δ given by the result of Problem No. 4 equal to the value for Δ above, and solve the equation for R_1, we have:

$$R_1 = R_3 = \frac{\frac{w\,l^4}{8\,I_3} + \frac{w}{8}\left(\frac{a^4}{I_1} - \frac{a^4}{I_2} + \frac{b^4}{I_2} - \frac{b^4}{I_3} + \frac{c^4}{I_3} - \frac{c^4}{I_4}\right)}{\frac{l^3}{3\,I_4} + \frac{1}{3}\left(\frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4}\right)}$$

$$= \frac{3\,w}{8}\,\frac{\frac{l^4}{I_4} + \frac{a^4}{I_1} - \frac{a^4}{I_2} + \frac{b^4}{I_2} - \frac{b^4}{I_3} + \frac{c^4}{I_3} - \frac{c^4}{I_4}}{\frac{l^3}{I_4} + \frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4}}$$

which is the expression for the value of the end reactions for a plate-girder of two equal spans having four values for the moment of inertia, and loaded uniformly throughout its length by w pounds per unit of length.

If $I_1 = I_2 = I_3 = I_4$, then $R_1 = R_3 = \frac{3}{8}\,w\,l$.

It would not be a difficult matter to derive values for the reactions for a more general case of loading and span lengths; but the ordinary

plate-girder draw has equal arms, and the only place, under the general practice in designing, where it is necessary to consider the effect of continuous loading is near and over the center support. The maximum values for shear and moment at the center support are given under conditions closely approximating continuous uniform load, and this is the problem we have solved.

In order to see how much the ordinary method (using a constant moment of inertia) of computing the moments over the center supports of plate-girder draws differs from this more exact method, the comparison will be made for two widely different cases.

Case I.—A very light, single-track, center-bearing, plate-girder draw, 72½ ins. deep, from out to out of flange-angles.

		Length over all.		Effective length.
1 web plate,	$72 \times \frac{3}{8}$ ins.	138 ft.	and	136 ft.
4 flange-angles,	5×5 "	138 "	"	136 "
(55 lbs. per yard.)				
2 cover-plates,	$12 \times \frac{5}{8}$ "	98 "	"	96 "
2 " "	$12 \times \frac{5}{8}$ "	22 "	"	20 "
2 " "	$12 \times \frac{5}{8}$ "	10 "	"	8 "

FIG. 7.

$I_1 = 38\,284$ ins.$^4 = 1.85$ ft.^{4} and $a = 20$ ft.
$I_2 = 58\,335$ " $= 2.81$ " $b = 58$ "
$I_3 = 79\,081$ " $= 3.81$ " $c = 64$ "
$I_4 = 100\,526$ " $= 4.85$ " $l = 68$ "

$$\frac{a^3}{I_1} = 4\,324.3, \quad \frac{a^3}{I_2} = 2\,847.0, \quad \frac{a^4}{I_1} = 8\,648.6, \quad \frac{a^4}{I_2} = 5\,694.0,$$

$$\frac{b^3}{I_2} = 69\,434.9, \quad \frac{f^3}{I_3} = 51\,210.5, \quad \frac{b^4}{I_2} = 4\,027\,206.3, \quad \frac{b^4}{I_3} = 2\,970\,209.0,$$

$$\frac{c^3}{I_3} = 68\,804.2, \quad \frac{c^3}{I_4} = 54\,050.3, \quad \frac{c^4}{I_3} = 4\,403\,468.8, \quad \frac{c^4}{I_4} = 3\,459\,219.8,$$

$$\frac{l^3}{I_4} = 64\,831.3, \qquad \frac{l^4}{I_4} = 4\,408\,531.1,$$

$$+ = 207\,394.7 \quad - = 108\,107.8 \quad + = 12\,847\,854.8 \quad - = 6\,435\,122.8$$

$$R_1 = \frac{3}{8} w \frac{\frac{l^4}{I_4} + \frac{a^4}{I_1} - \frac{a^4}{I_2} + \frac{b^4}{I_2} - \frac{b^4}{I_3} + \frac{c^4}{I_3} - \frac{c^4}{I_4}}{\frac{l^3}{I_4} + \frac{a^3}{I_1} - \frac{a^3}{I_3} + \frac{b^3}{I_3} - \frac{b^4}{I_3} + \frac{c^4}{I_3} - \frac{c^4}{I_4}}$$

$$= \frac{3}{8} w \frac{12\,847\,854.8 - 6\,435\,132.8}{207\,394.7 - 108\,107.8} = \frac{3}{8} w\, 64.59 = 24.22\, w.$$

The moment over the center support for this case is:

$M = 68\ w\ (24.22 - 34.00) = -\ 665.04\ w$ foot-pounds.

The moment over the center support for constant moment of inertia is:

$$M = -\frac{1}{8}\ w\ l^2 = -\ 578\ w \text{ foot-pounds,}$$

which shows that this more exact method gives a moment over the center support for this case 15% greater than the usual method.

Case II.—A very heavy, double-track, center-bearing, plate-girder draw, 101½ ins. deep, from out to out of flange angles.

		Length over all.		Effective length.
1 web plate,	$101 \times \frac{11}{16}$ ins.,	159 ft.	and	156 ft.
4 side flange-plates,	$18 \times \frac{3}{4}$ "	159 "	"	156 "
4 flange angles,	8×6 "	159 "	"	156 "
	(125 lbs. per yard.)			
2 cover-plates,	$20 \times \frac{3}{4}$ "	159 "	"	156 "
2 " "	$20 \times \frac{3}{4}$ "	84 "	"	82 "
2 " "	$20 \times \frac{3}{4}$ "	24 "	"	22 "
2 " "	$20 \times \frac{3}{4}$ "	16 "	"	14 "

$I_1 = 353\ 750$ ins.^{4} $= 17.06$ ft.^{4} and $a = 37$ ft.

$I_2 = 434\ 500$ " $= 20.96$ " " $b = 67$ "

$I_3 = 517\ 600$ " $= 24.96$ " " $c = 71$ "

$I_4 = 603\ 100$ " $= 29.08$ " " $l = 78$ "

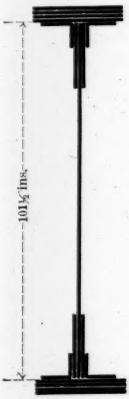

FIG. 8.

$$\frac{a^3}{I_1} = 2\ 969.1,\ \frac{a^3}{I_2} = 2\ 416.7,\ \frac{a^4}{I_1} = 109\ 857.3,\ \frac{a^4}{I_2} = 89\ 416.1,$$

$$\frac{b^3}{I_2} = 14\ 349.4,\ \frac{b^3}{I_3} = 12\ 049.9,\ \frac{b^4}{I_2} = 961\ 408.5,\ \frac{b^4}{I_3} = 807\ 336.6,$$

$$\frac{c^3}{I_3} = 14\ 339.3,\ \frac{c^3}{I_4} = 12\ 307.8,\ \frac{c^4}{I_3} = 1\ 018\ 096.2,\ \frac{c^4}{I_4} = 873\ 854.2,$$

$$\frac{l^3}{I_4} = 16\ 318.8, \qquad \frac{l^4}{I_4} = 1\ 272\ 869.9,$$

$$+ = 47\ 976.6 \quad - = 26\ 774.4 \quad + = 3\ 362\ 231.9 \quad - = 1\ 770\ 606.9$$

$$R_1 = \frac{3}{8}\ w\ \frac{3\ 362\ 231.9 - 1\ 770\ 606.9}{47\ 976.6 - 26\ 774.4} = \frac{3}{8}\ w\ 75.07 = 28.15\ w.$$

The moment over the center support, for this case, is:

$M = 78\ w\ (28.15 - 39.00) = -\ 846.30\ w$ foot-pounds.

The moment over the center support, for constant moment of inertia, is:

$$M = -\frac{1}{8} w l^2 = 760.5\ w \text{ foot-pounds.}$$

Which shows that this more exact method gives a moment over the center support for this case 11% greater than the usual method.

The two plate-girder drawbridges selected for investigation may be said to represent fairly the results obtained by the method commonly used in designing such structures. In view of the results of the investigation for these two cases, it is fair to assume that many center-bearing plate-girder drawbridges are stressed considerably higher, for a short distance over the center support, than their designers intended they should be. This condition of affairs, it is believed, is quite generally known, but, as far as the writer knows, has not influenced designers. The additional material required for a more correct design is a very small percentage of the whole weight, and, considering the increased strength it would give, the addition should always be made.

The objection may be made that it is hardly necessary to go to any great refinement in designing plate-girder drawbridges, as it is not certain that the end machinery will act as designed, and, further, even though the condition of the supports be as assumed, until account is taken of the effect of the shearing stresses in producing distortion, the true reactions cannot be obtained. The fact that the end supports may be out of their proper position, and in such a manner as to cause increased bending moments over the center support, is an argument in favor of the most refined methods in designing the structure. It is not customary to consider the effect of the shearing stresses in producing distortion, in designing plate-girder drawbridges; the method used in investigating Case I and Case II is simply a more refined application of the usual method (which considers only flexural stresses). In order to determine the effect of the shearing stresses in producing deflection, the following investigation is made:

Let us take any portion of a beam in equilibrium under the action of transverse loading, as shown in Fig. 9. If P be the resultant in position, direction and amount of all the forces to the right of the section $m\ n$, then it is clear that

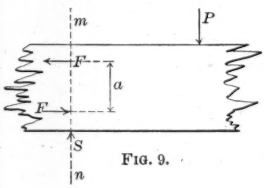

FIG. 9.

the portion of the beam under consideration would be kept in equilibrium by the couple, Fa, and the shear, S.

In general, no matter what may be the actual distribution of the stresses on any section of a beam under a given loading, these stresses may be resolved into an equivalent couple and shear. The effect of the couple in doing work has been determined in accordance with the common theory of flexure. Going back to (1), and rewriting the expression for the work of the internal stresses, we have

$$w = \left(\int_o^l \frac{Mm\,dx}{2EI} \text{ for the work of the flexural stresses}\right)$$
$$+\left(\int_o^l \frac{Ss\,dx}{2E_sA} \text{ for the work of the shearing stresses}\right).$$

To derive the expression for the work done by the shearing stresses:

Let S = shear at any point, due to the given loading;

s = shear at any point, due to a load of unity at the point where deflection is desired;

A = total area of the cross-section;

E_s = coefficient of elasticity for shear;

and let dx be an infinitesimal portion of the length of the beam; that is, let it be considered as the distance between two consecutive sections of the beam.

Then, under the action of the shearing stress, S, the two consecutive sections will have a relative motion of $\frac{S\,dx}{A\,E_s}$, if we assume that the shearing stress is distributed uniformly over the cross-section.

Now, as for the flexural stresses, we will suppose the force of unity to be applied gradually; then the work done by the shearing stress, on an elementary portion of the beam, due to the force of unity, is $\frac{S\,dx}{A\,E_s} \times \frac{s}{2}$, and, over the entire beam, the work is $\int_o^l \frac{Ss\,dx}{2E_sA}$.

Making the expressions for the external and internal work equal, we have:

$$\frac{\Delta}{2} = \int_o^l \frac{Mm\,dx}{2EI} + \int_o^l \frac{Ss\,dx}{2E_sA}, \text{ and}$$

$$\Delta = \int_o^l \frac{Mm\,dx}{EI} + \int_o^l \frac{Ss\,dx}{E_sA}.$$

The shearing stress is not distributed uniformly over the cross-section, and hence the term for the work of the shearing stress should

be modified by a coefficient. The value of this coefficient for a rectangular section is very readily shown to be $\frac{6}{5}$.

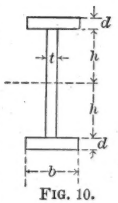

FIG. 10.

For a section, as shown in Fig. 10, which is a close enough approximation to plate-girder cross-sections, for all practical purposes, the work done by the shearing stresses due to unity, taking the intensity of the vertical shear as equal to the intensity of the horizontal shear at all points of the cross-section, on two flanges

$$= \frac{b\,[8\,(h+d)^5 - 15\,(h+d)^4\,h + 10\,(h+d)^2\,h^3 - 3\,h^5]}{30\,I^2} \int_o^l \frac{S\,s\,d\,x}{E_s}$$

and on the web

$$= \frac{30\,b^2\,d^2\,\frac{h}{t}\left(h+\frac{d}{2}\right)^2 + 20\,b\,d\,h^3\left(h+\frac{d}{2}\right) + 4\,h^5\,t}{30I^2} \int_o^l \frac{S\,s\,d\,x}{E_s}$$

The total work on the entire cross-section is the sum of these, and

$$8\,b\,(h+d)^5 - 15\,b\,h\,(h+d)^4 + 10\,b\,h^3\,(h+d)^2 - 3\,b\,h^5 + 30\,b^2\,d^2\,\frac{h}{t}\left(h+\frac{d}{2}\right)^2 + 20\,b\,d\,h^3\left(h+\frac{d}{2}\right) + 4\,h^5\,t = \frac{}{30\,I^2} \int_o^l \frac{S\,s\,d\,x}{E_s}$$

This expression is of such an involved nature that a simple inspection gives very little information as to the relative value of the work on the flanges and web to that of the work on the entire cross-section. For the purpose of getting a definite idea of this relation, and, at the same time, for reinvestigating the plate-girders of Case I and Case II, approximate sections for the end and center of each of these girders will be assumed.

Case I.

Approximate end-section:	Approximate center-section:
$b = 12$ ins.	$b = 12$ ins.
$d = 1$ "	$d = 2$ "
$h = 36$ "	$h = 36$ "
$t = \frac{3}{8}$ "	$t = \frac{3}{8}$ "
Work on flanges = 0.0003	Work on flanges = 0.0008
Work on web = 0.9997	Work on web = 0.9992

Case II.

Approximate end-section:	Approximate center-section:
$b = 20$ ins.	$b = 20$ ins.
$d = 2$ "	$d = 4$ "
$h = 50$ "	$h = 50$ "
$t = \frac{3}{4}$ "	$t = \frac{3}{4}$ "
Work on flanges $= 0.0006$	Work on flanges $= 0.0013$
Work on web $= 0.9994$	Work on web $= 0.9987$

From this it is seen that the amount of work done on the flanges is a very small part of the total work on the cross-section. A similar investigation on a section more closely approximating the actual cross-section of a plate-girder, as shown in Fig. 11, would show a considerably larger amount of work for the flanges; but it would still be very small. Therefore, in investigating the effect of shear in producing distortion on plate-girders it will be very nearly correct to assume that the web alone resists all the shearing stresses. This assumption would give deflections a little too great, as the flanges of a plate-girder do take a small part of the shear—for a plate-girder such as that in Case II the vertical flange plates and the vertical legs of the flange-angles take considerable shear; however, in the investigation to follow, the effect of the unequal distribution of the shear over the web will be neglected.

FIG. 11.

This assumption produces an error opposite to and generally greater than the first assumption, so that the resulting deflections determined under these assumptions will be very nearly correct, but probably a trifle too small.

The general equation for the deflection of a plate-girder of varying cross-section now becomes

$$\Delta = \int_o^l \frac{M\,m\,dx}{E\,I} + \int_o^l \frac{S\,s\,dx}{E_s\,A_w}, \text{ in which } A_w = \text{area of web.}$$

Making $E_s = \frac{2}{5}\,E$, the deflection

$$\Delta = \int_o^l \frac{M\,m\,dx}{E\,I} + \frac{5}{2}\int_o^l \frac{S\,s\,dx}{E\,Aw}.$$

From this the expression for the value of the deflection for Problem No. 4 becomes

$$\Delta = \frac{w}{8\,E}\left(\frac{l^4}{I_4} + \frac{a^4}{I_1} - \frac{a^4}{I_2} + \frac{b^4}{I_2} - \frac{b^4}{I_3} + \frac{c^4}{I_3} - \frac{c^4}{I_4}\right) + \frac{5}{4}\frac{w\,l^2}{E\,A_w};$$

and for Problem No. 5 it becomes

$$\Delta = \frac{P}{3\,E}\left(\frac{l^3}{I_4} + \frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^3}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4}\right) + \frac{5}{2}\frac{w\,l^2}{E\,A_w}$$

The expression for the value of the end reactions for a plate-girder of two equal spans, the girder having four values of I, and loaded uniformly throughout its length by w pounds per unit of length, is:

$$R_1 = R_3 = \frac{3}{8}\,w\,\frac{\left(\frac{l}{I_4} + \frac{a^4}{I_1} - \frac{a^4}{I_2} + \frac{b^4}{I_2} - \frac{b^4}{I_3} + \frac{c^4}{I_3} - \frac{c^4}{I_4}\right) + \frac{10\,l^2}{A_w}}{\left(\frac{l^3}{I^4} + \frac{a^3}{I_1} - \frac{a^3}{I_2} + \frac{b^4}{I_2} - \frac{b^3}{I_3} + \frac{c^3}{I_3} - \frac{c^3}{I_4}\right) + \frac{15\,l}{A_w}}.$$

Using this expression in investigating Case I and Case II:

For Case I:

$$R = \frac{3}{8}\,w\,\frac{6\,412\,732 + 246\,613}{99\,287 + 2\,720} = \frac{3}{8}\,w \times 65.28 = 24.48\,w;$$

and the moment over the center support,

$$M = 68\,w\,(24.48 - 34.00) = -647.36\,w \text{ foot-pounds.}$$

This moment is only 2.6% less than that found by the first investigation for Case I.

For Case II:

$$R = \frac{3}{8}\,w\,\frac{1\,591\,625 + 126\,165}{21\,202 + 1\,213} = \frac{3}{8}\,w \times 76.64 = 28.74\,w;$$

and the moment over the center support,

$$M = 78\,w\,(28.74 - 39.00) = -800.28\,w \text{ foot-pounds.}$$

This moment is 5.5% less than that found by the first investigation for Case II.

The result of this reinvestigation for Case I and Case II shows. For the shorter and lighter draw-span, that the deflection of the end of the span, due to the shearing stresses caused by uniform loading, is about 4% of that due to the flexural stresses from the same cause; that the deflection of the end of the span due to the shear caused by a load at the end is about 3% of that due to the flexural stresses from the same cause; and that the calculated moment over the center support, obtained by a consideration of the flexural stresses alone, is about 2.6% greater than that obtained by taking into account the effect of the shearing stresses.

For the longer and heavier draw-span, the consideration of the effect of the shearing stresses shows differences about twice as great, in each instance, as was shown for the shorter and lighter structure.

That the effect of the shearing stresses should be greatest in the long girder, does not seem to be in accord with the fact that, for a beam of constant cross-section, the relative effect of the shearing stresses is less the longer the span; an inspection of all the elements of the problem shows that the much larger moment of inertia and the relatively less area in the web of the long girder are more than sufficient to overcome the effect of its greater length. In view of the results of this investigation it would seem to be necessary to consider the effect of the shearing stresses, in modifying the effect of the flexural stresses in designing, only for very deep girders subject to very heavy loads. The amount of labor required for a proper consideration of the shearing stresses is very small, so that whenever great accuracy is required they should always be considered.

DISCUSSION.

Mr. Church. IRVING P. CHURCH, Assoc. Am. Soc. C. E. (by letter).—The author has presented a derivation of the "Fraenkel Formula,"

$$\Delta = \int_o^l \frac{M m \, d\,x}{E\,I}, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

which seems to the writer to be lacking in rigor; or at least would probably be so considered by a reader not familiar with the books on "Least Work" by Castigliano and Martin, and who, therefore, might be unable to "read between the lines."

For mathematical purposes, an auxiliary force is supposed to be applied to the beam at the point whose deflection is desired, in addition to the actual loading. Now, this force should ultimately be reduced to zero (as in similar demonstrations by Castigliano) before a result can be reached that will be rigorously applicable to the beam under actual loading alone. Such an ultimate reduction to zero is presumably implied by the author in the evident assumption that the effect of the bending moment due to the auxiliary force in the production of the change of length, λ, in fiber, is negligible. Strictly, if M' denote the bending moment due to the auxiliary force alone, we should have

$$\lambda = \frac{(M + M')\, y\, d\,x}{E\,I}; \text{ instead of } \frac{M\, y\, d\,x}{E\,I}.$$

To ignore the M', then, when added to the M (moment due to actual loading), is to anticipate the final reduction to zero of the auxiliary force. But, in the demonstration, as presented by the author, the auxiliary force used is one of unity, which, of course, cannot be reduced to zero; and nothing is said as to any change of value on the part of this force.

To the writer, this obscurity seems entirely unnecessary, and he would suggest the following changes in the details of the argument: Let the auxiliary force at the point in question have a value of Q units; and let Δ' be the deflection, in the direction of this force, of the point of application; as due to the combined action of Q and the actual loading.

Under these circumstances, if M' is the bending moment at any section as due to Q alone, the final stress in any fiber, as due to Q alone, will be $\frac{M'y}{I}$; and hence, eventually, the internal work due to Q alone, in the simultaneous and gradual application of both Q and the actual loading, will be, for the whole beam

$$W = \int_o^l \frac{(M + M')\, M'\, d\,x}{2\,E\,I}; \quad \ldots\ldots\ldots\ldots\ldots\ldots (1a)$$

while the external work of Q will be

$$W' = \frac{Q\,\Delta'}{2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2a)$$

Equating these values of W and W', we have

$$\frac{Q\,\Delta'}{2} = \int_o^l \frac{(M + M')\,M'\,d\,x}{2\,E\,I}.$$

If, now, we define a quantity, m, as being the rate at which Q produces the bending moment, M', in any section; in other words, if we state that the value of M' is at the rate of m foot-pounds moment for each pound (or other unit) in the value of Q, we may write $M' = Q\,m$; whence

$$\frac{Q\,\Delta'}{2} = Q \int_o^l \frac{(M + Q\,m)\,m\,d\,x}{2\,E\,I} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2b)$$

In this expression it is to be specially noted that m is a function simply of the position of Q (being numerically equal to the bending moment that would be produced at any section by a force of unity at the point in question), but that it is not a function of the magnitude of Q.

Equation 2*b* may now be written

$$\Delta' = \int_o^l \frac{(M + Q\,m)\,m\,d\,x}{E\,I}, \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2c)$$

and is evidently available for determining the deflection of the point of application of Q, in direction of Q, as due to both the original loading and the force, Q, itself; Q having any value. If, now, Q be made equal to zero, the equation takes the form

$$\Delta = \int_o^l \frac{M\,m\,d\,x}{E\,I}; \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

the quantity, m, retaining its first value, since it is independent of the magnitude of Q, while Δ denotes the deflection of the point in question, as due to the original loading alone. In this way it would seem that all obscurity in the derivation of the Fraenkel Formula might be avoided. It can easily be shown, however, that Equation 2*c* is, after all, nothing more than an expression of the fact or theorem, proved by Castigliano and others, that the deflection or displacement (in the direction of the force) of the point of application of a force, Q, acting with other forces on an elastic beam, is equal to the derivative (or "differential coefficient") of the total internal work of the beam, with respect to the force, Q. For it will be noticed that, if M'' be used to denote the bending moment at any section, as due to all the loads, including Q (that is, let $M'' = M + Q\,m$), we have $\frac{d\,M''}{d\,Q} = m$; and hence Equation 2*c* becomes

$$\Delta' = \int_o^l \frac{M''}{E\,I}\left(\frac{d\,M''}{d\,Q}\right) d\,x \quad \ldots\ldots\ldots\ldots\ldots\ldots (2d)$$

Mr. Church. Now, the total internal work of flexure for the beam (as far as bending moments are concerned) is known to be

$$W'' = \int_o^l \frac{M''^2 \, dx}{2EI};$$

and hence, by Castigliano's Theorem, just mentioned, we have, for the deflection, Δ', at the point of application of Q,

$$\Delta' = \frac{d\,W''}{d\,Q};$$

that is,

$$\Delta' = \int_o^l \left[2\,M'' \frac{d\,M''}{d\,Q}\right] \frac{dx}{2EI}, \; = \; \int_o^l \frac{M''}{EI}\left(\frac{d\,M''}{d\,Q}\right) dx\,; \ldots\ldots (2e)$$

which is seen to be identical with Equation 2*d*.

It follows, therefore, that the quantity, m, while numerically equal to the bending moment that would be due to a force of unity at the point of application of Q and having the same direction, is simply the derivative of the total bending moment at any section, with respect to Q. As already noted, this derivative is independent of the magnitude of Q (M'' being a linear function of Q), and hence has the same value, when Q becomes zero, as for any other magnitude. It is thus seen that the "Fraenkel Formula" is a particular case of "Castigliano's Theorem."

While the Fraenkel Formula is doubtless the most advantageous for the problems treated in this paper, it may be of interest to call attention to the fact that the Theorem of Three Moments (with variable moment of inertia), may be used for the same purpose (if the influence of shear be disregarded), for the reason that in its most general form this theorem relates to any three points in the elastic curve of a bent beam (whether or not there are supports at any of these three points); and that it introduces the deflection of the intervening point below the straight line joining the other two. When the theorem is applied to Problems Nos. 1, 2 and 3, of the paper, the three points chosen would be the two extremities of the beam (over the supports), and the point whose deflection is desired. Under these circumstances, the desired deflection is identical with that which appears in the statement of the theorem, and the end moments are each zero; so that a solution is immediately effected for the deflection. A similar obvious application may be made for Problems Nos. 4 and 5.

Mr. Merriman. MANSFIELD MERRIMAN, M. Am. Soc. C. E.—The speaker has read this paper with much interest, and regards it as a valuable contribution to the theory of beams. The general method of determining deflections of beams with variable moments of inertia has long been known, but this appears to be the first deduction which includes the shearing resistance of the web, as well as the tensile and compressive resistances in the flanges. The formulas, although apparently lengthy,

are not difficult of application, and it is now possible to make precise computations of the deflection of the ends of a plate-girder drawbridge with variable cross-sections. Mr. Merriman.

The early investigators regarded the influence of the shearing stresses on the deflection as too small to be considered. For a long beam of constant cross-section this is often the case, but when the beam is short the influence of shearing may be very marked. A plate-girder is a case where the shearing effects should not be neglected, and it appears that the influence of variable cross-section produces changes in the reactions and bending moments which are unexpected. It may be noted, moreover, that the influence of shear seems to increase with investigation; every extended analysis seeming to make it slightly greater than before.

The work in the flanges due to shearing is but a small part of that in the web, as the author's investigation fully shows, and hence it may properly be neglected. The author's treatment of the work in the web, regarding the shear to increase toward the neutral axis, is in the right direction, but after this was done it is not clear why the coefficient, $\frac{2}{3}$, was not retained in the general formula on page 15. Neglect of the shear in the flanges does not appear to be balanced by neglect of the actual distribution of the shear in the web. The effect of these assumptions on the deflection is not great, but the effect on the reactions and bending moments in the cases considered may perhaps be a considerable one. The author's very excellent investigation cannot, therefore, be regarded as final in regard to the influence of the shearing stresses.

In reference to this it may be observed that the common theory of beams, in which the stresses are represented by horizontal tensions and compressions and by vertical shears, is an approximation only. At any point in the beam there really act tensile and compressive stresses at right angles to each other, but these are not horizontal, except at the upper and lower surfaces, while the shearing stresses are not vertical, but oblique. The actual condition is hence one of great complexity, and it may be considered as surprising that the common theory gives results for common cases agreeing so closely with practice. In extreme cases, however, the theory must be supplemented to meet the unusual conditions. The paper, therefore, treats, not only with a practical engineering problem, but with an important scientific question.

C. H. Lindenberger, Assoc. Am. Soc. C. E. (by letter).—The writer is glad to see that Mr. Hudson is urging reform in the computation of girders with variable moments of inertia. Mr. Lindenberger.

What is wanted is mainly, not new formulas, but simpler methods of applying those already in existence; for, in the writer's experience, it requires only a small amount of mental confusion to discourage the

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ordinary bridge computer. This is not a fault of intellect, but is due to the fact that computation is a task, and not a luxury, with him, and he is obliged to crowd a great deal into a day's work, under circumstances which practically prohibit studying out abstruse problems.

The writer was also much gratified to find that Mr. Hudson's formula, originally due to Professor Fraenkel, agrees with that of the writer, published in 1891. The agreement is not a mere general agreement, but a precise and exact identity, in any and every particular when concentrated loads and vertical deflections only are considered. The fact of this identity can be demonstrated analytically, by developing the integral, $\int \frac{M\,m\,dx}{E\,I}$, for the cases given; but the question is of only theoretical importance, and hardly interesting enough to print in the *Transactions*.

The formula which the writer calls his is merely the old three-moment theorem applied to girders with variable moments of inertia, and is originally due to Professor Weyrauch in his "Continuirlichen und Einfachen Träger." The writer is indebted to Malverd A. Howe, M. Am. Soc. C. E., for his knowledge of the subject, as given in Mr. Howe's "Continuous Girder." This formula* is a rearrangement and simplification of these others that preceded it, and, of course, gives, absolutely and rigorously, identical results. The integrals for uniform load were not developed.

The problem of simplification, for the hurried and busy computer, has about reached its limit, as far as analysis is concerned. All that remains is to calculate a case, with all its numerical details, and show that Greek letters and signs of summation are not nearly as complicated as they appear to be.

It will be shown that not only is the formula complete, as a method of calculation, but also, that every single detail of the process can be checked by a graphical method, and this is not possible by any other formula.

The girder selected is the one detailed by Mr. Hudson: 101½ ins. deep and 156 ft. long. It will be supposed to rest on two supports, and the deflection at a point 70 ft. from the left end is wanted. Now, it may be considered as a girder continuous over three supports, provided it be assumed that the support at the deflection point has sunk, so that the girder just touches, and the reaction is zero.

Then l_1 = first span = 70 ft.;

l_2 = second span = 86 ft.;

ε_v and ε_{v-1} are the horizontal co-ordinates of two points, respectively, of this girder, between which the moment of inertia is constant;

I_v is the moment of inertia between these points;

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M_2 is the moment at the deflection point, and is positive when there is tension on the upper flange;

E is the modulus of elasticity = 4 176 000 000 lbs. per square foot.

$$\Delta\ \varepsilon_v = \frac{\varepsilon_v - \varepsilon_{v-1}}{I_v}$$

$$\Delta\ \varepsilon^2_v = \frac{\varepsilon_v{}^2 - \varepsilon^2_{v-1}}{I_v}$$

$$\Delta\ \varepsilon^3_v = \frac{\varepsilon^3_v - \varepsilon^3_{v-1}}{I_v}$$

$$E\,\gamma_v = \frac{\Delta\ \varepsilon^3_v}{3\,l^2_r}$$

$$E\,\lambda_v = \frac{\Delta\ \varepsilon^2_v}{2\,l_r} - \frac{\Delta\ \varepsilon^3_v}{3\,l^2_r}$$

$$E\ \tau_v = \Delta\ \varepsilon_v - \frac{\Delta\ \varepsilon^2_v}{l_r} + \frac{\Delta\ \varepsilon^3_v}{3\,l^2_r}.$$

Σ is the sign of summation.

Take the sum of the above from end to end of the rth span; then

$$G_r = \sum_o^l \gamma_v, \qquad L_r = \sum_o^l \lambda_v, \qquad T_r = \sum_o^l \tau_v.$$

P_r is a load in the rth span. In this case $P_1 = 40\,000$ lbs.; $P_2 = 20\,000$ lbs.

A_r is the distance of a weight in the rth span from its left support. In this case $a_1 = 47$ ft., and $a_2 = 25$ ft.

$$\frac{B_1}{P_1} = (l_1 - a_1) \sum_o^{a_1} \gamma_v + a_1\,(L_1 - \sum_o^{a_1} \lambda_v);$$

$$\frac{A_2}{P_2} = (l_2 - a_2) \sum_o^{a_2} \lambda_v + a_2\,(T_2 - \sum_o^{a_2} \tau_v);$$

h_2 is the deflection, positive downward.

The following definitions relate only to the graphical method, but it is necessary to give some idea of them here, in order to explain the necessity for a change in the arrangements of the moments of inertia.

θ, m, and J, are purely arbitrary quantities used only in the graphical calculation.

$\theta = 6\,E$ multiplied by an arbitrary moment of inertia, here taken as 30 ft. (four dimensions). Its use is to eliminate E and to obtain linear quantities. Hence $\theta = 180\,E$.

It may seem somewhat queer to say that the unit of force is also the unit of length, viz., 1 ft.; but this is what must be done here, since E is eliminated. Everything is measured in feet.

J is the pole distance of a force polygon for forces as described below. It is here taken at 30 ft.

m is the width of a right parallelogram, the length of which is the length of the span, and is here assumed to be 10 ft. This parallelogram is divided into two main triangles by one of its diagonals. Each

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of these triangles is divided by vertical lines, at the points where the moments of inertia change values, into smaller subdivisions, viz., an end triangle and one or more trapezoids. The areas of these smaller subdivisions divided by their respective corresponding girder's moment of inertia, are reduced to linear quantities and are considered as vertical forces acting through the center of gravity of the areas from which they are derived. The moments of these forces, with respect to the supports, are proportional either to γ_p, λ_p or τ_p, according to which support, or which half of the parallelogram, is taken. The moments of the forces, and therefore γ_p, λ_p and τ_p, are found by constructing an equilibrium polygon and prolonging the bounding lines to the verticals through the supports, the same as any ordinary equilibrium polygon.

It will be plain from this that to obtain G_r, L_r and T_r the end sides of this equilibrium polygon are prolonged to the verticals through the supports. Now, if the arrangement as given by Mr. Hudson is examined, it will be found that part of the girder, the moment of inertia of which is 24.96, has only 3 ft. in the first span and 1 ft. in the second span. The sides of the equilibrium polygons at the right end of the first span and the left end of the second span would therefore be entirely too short to prolong to the verticals through the supports. In the first span, therefore, two parts are consolidated, and the moment of inertia used $= \frac{20.96 \times 30 + 24.96 \times 3}{33} = 21.32$. Again, in the second span we have the 24.96 section, on the left, 1 ft. long; then the 29.08 section, 14 ft. long; then another 24.96 section, 4 ft. long. In order to make the drawing clear and distinct all these were consolidated into a section the moment of inertia of which is $\frac{24.96 \times 5 + 29.08 \times 14}{19} = 28$. In practical work, on a larger scale, it would not be necessary to go to these extremes in either span; but in a drawing, in which the subject is illustrated and explained, everything should be clear and distinct.

The error due to these assumptions has been duly calculated, by the tabular method explained below, and is only about $\frac{1}{40}$ of 1 per cent.

The theorem of three moments* assumes the following form for this case:

$$h_2 = \frac{l_1 l_2}{l_1 + l_2}\Big(-M_2 (G_1 + T_2) + A_2 + B_1\Big).$$

Moments causing tension on the upper flange are considered positive, and deflections are positive downward.

Since this girder is in reality only a simple girder with reaction zero at the middle support, we can at once compute M_2 thus: The

* *Journal* of the Franklin Institute, January, 1891.

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reaction at the left end of the girder

$$= \frac{1}{156}\Big(20\,000 \times 61 + 40\,000 \times 109\Big) \text{ lbs.} = 35\,769 \text{ lbs.}$$

$$M_2 = -(35\,769 \times 70 - 40\,000 \times 23) = -1\,583\,830,$$

measured in foot-pounds.

We will now proceed to calculate G_1, L_1 and B_1 from the following table:

First Span.

v	ϵ_v	I_v	$\Delta\,\epsilon^2{}_v$	$\Sigma\,\Delta\,\epsilon^2{}_v$	$\Delta\,\epsilon^3{}_v$	$\Sigma\,\Delta\,\epsilon^3{}_v$
1.....	37	17.06	80.2462	.....	2 969.1090	.....
2.....	70	21.32	165.6191	245.8653	13 712.3358	16 681.4448

$$E\,G_1 = \frac{16\,681.4448}{14\,700} = 1.1348;$$

$$E\,L_1 = \frac{245.8653}{140} - \frac{16\,681.4448}{14\,700} = 1.7562 - 1.1348 = 0.6214.$$

To compute $\sum_{o}^{a_1} \gamma_v$ and $\sum_{o}^{a_1} \lambda_v$, we must find the functions like those in the table between the points, $\varepsilon = a_1 = 47$, and $\varepsilon = \varepsilon_1 = 37$; thus,

$$\Delta\,\varepsilon^2 = \frac{(47)^2 - (37)^2}{21.32} = 39.3996;$$

$$\Delta\,\varepsilon^3 = \frac{(47)^3 - (37)^3}{21.32} = 2\,493.9024.$$

$$E\sum_{o}^{a_1} \gamma_v = \frac{2\,969.1090 + 2\,493.9024}{14\,700} = 0.3716;$$

$$E\sum_{o}^{a_1} \lambda_v = \frac{80.2462 + 39.3996}{140} - \frac{2\,969.1090 + 2\,493.9024}{14\,700}$$

$$= 0.8546 - 0.3716 = 0.4830.$$

Hence,

$$E\,B_1 = 40\,000\,[23 \times 0.3716 + 47\,(0.6214 - 0.4830)] = 602\,064.$$

The next is the calculation of L_2, T_2, and A_2, which is done from the following table:

Second Span.

v	ϵ_v	I_v	$\Delta\,\epsilon_v$	$\Sigma\,\Delta\,\epsilon_v$	$\Delta\,\epsilon^2{}_v$	$\Sigma\,\Delta\,\epsilon^2{}_v$	$\Delta\,\epsilon^3{}_v$	$\Sigma\,\Delta\,\epsilon^3{}_v$
1...	19	28.	0.6786	.....	12.8929	.....	244.9643	.....
2...	49	20.96	1.4313	2.1099	97.3282	110.2211	5 285.7824	5 530.7467
3...	86	17.06	2.1688	4.2787	292.7902	403.0113	30 387.2802	35 918.0269

$$E\,L_2 = \frac{403.0113}{172} - \frac{35\,918.0269}{22\,188} = 2.3431 - 1.6188 = 0.7243;$$

$$E\,T_2 = 4.2787 - \frac{403.0113}{86} + \frac{35\,918.0269}{22\,188} = 4.2787 - 4.6862 + 1.6188$$

$$= 1.2113.$$

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In order to compute A_2, we must compute $\Delta\,\varepsilon$, $\Delta\,\varepsilon^2$, and $\Delta\,\varepsilon^3$ for that part of the girder between $\varepsilon = a_2 = 25$ ft., and $\varepsilon = 19$ ft., thus:

$$\Delta\,\varepsilon = \frac{25 - 19}{20.96} = 0.2863;$$

$$\Delta\,\varepsilon^2 = \frac{(25)^2 - (19)^2}{20.96} = 12.5954;$$

$$\Delta\,\varepsilon^3 = \frac{(25)^3 - (19)^3}{20.96} = 418.2252;$$

$$E\sum_{o}^{a_2}\lambda_v = \frac{12.8929 + 12.5954}{172} - \frac{244.9643 + 418.2252}{22\,188}$$

$$= 0.1482 - 0.0299 = 0.1183;$$

$$E\sum_{o}^{a_2}\tau_v = 0.6786 + 0.2863 - \frac{12.8929 + 12.5954}{86} + \frac{244.9643 + 418.2252}{22\,188}$$

$$= 0.6786 + 0.2863 - 0.2964 + 0.0299 = 0.6984;$$

$$E\,A_2 = 20\,000\,[61 \times 0.1183 + 25\,(1.2113 - 0.6984)] = 400\,776.$$

Substituting all values in the three-moment theorem, previously mentioned, as applied to this problem, we get

$$h_2 = \frac{70 \times 86\,[1\,583\,830\,(1.1348 + 1.2113) + 400\,776 + 602\,064]}{(70 + 86)\,4\,176\,000\,000}\ \text{ft.}$$

$$= 0.0436\ \text{ft.} = 0.5232\ \text{in.}$$

In the girder, as designed by Mr. Hudson, the deflection would be 0.04359 ft.

The reader who examines the original article, in the *Journal* of the Franklin Institute, will find some changes introduced which the writer believes will facilitate calculation. In particular, instead of θ_{v-1}, which is rather confusing,* the writer has substituted $6\,E\,I_v$ here, which represents the same idea, and has changed the formula so that no double work is done by first multiplying by and then dividing by $6\,l^2_r$, $6\,l_r$, etc.

While the above completes the tabular method, there are some data which are useful in checking up the graphical method; or, *per contra*, in checking the tabular method from the graphical. The figures of the tabulation may be checked from the graphical method, as will be shown later.

From the table for the "first span" we get:

$E\,\gamma_1 = 0.202$; $E\,\lambda_1 = 0.3712$;
$E\,\gamma_2 = 0.9328$; $E\,\lambda_2 = 0.2502$.

Also, from the table for the "second span":

$E\,\lambda_1 = 0.064$; $E\,\tau_1 = 0.5397$;
$E\,\lambda_2 = 0.3276$; $E\,\tau_2 = 0.5378$;
$E\,\lambda_3 = 0.3327$; $E\,\tau_3 = 0.1338$.

The following is an explanation of the graphical method. Fig. 12 represents the method of calculating L_1, G_1 and B_1. In Fig. 13, L_2, T_2 and A_2 are computed. The upper part, in each, shows the girder,

* See Howe's "Continuous Girder" for subscript copied.

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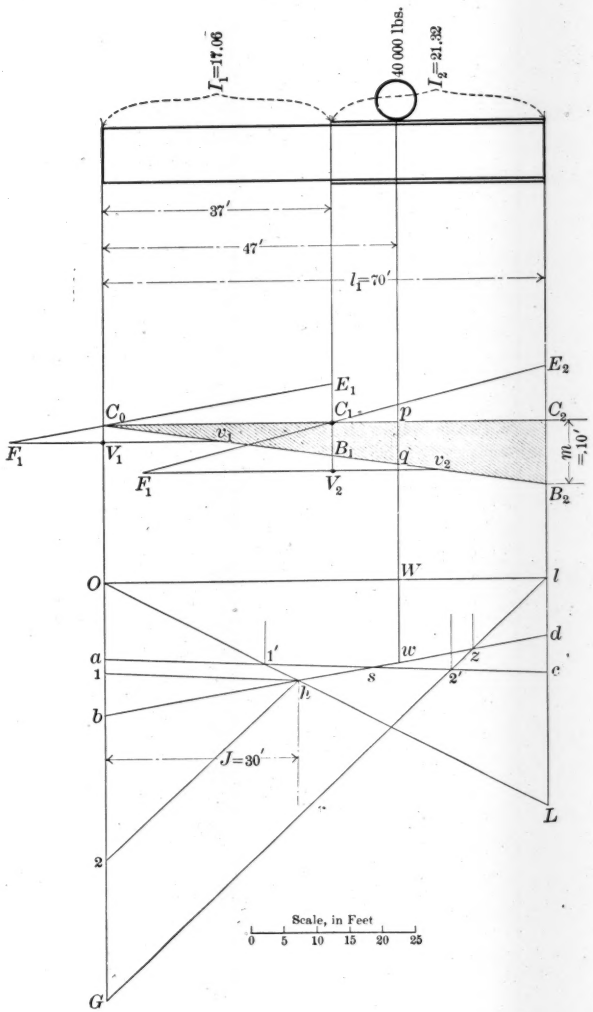

FIG. 12.

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its moments of inertia, and its load. Below this is the triangle, half of the parallelogram previously mentioned. We are to calculate the required quantities from the areas of the subdivisions of this triangle. Underneath the triangle is the equilibrium polygon, and on the left is the force polygon, the pole distance of which is J (= 30 ft.), both of which have also been described previously in a general way. It should be noted that in Fig. 12 the toe of the triangle is on the left side, while in Fig. 13 it is the reverse.

If v is the number of any section, either in Fig. 12 or Fig. 13, then the formula for the ordinate, $E_v\ C_v$ is

$$E_v\ C_v = \frac{E\ I_v\ l_v}{\theta};$$

and

$$\theta = 180\ E.$$

We will commence with these in Fig. 12:

$$E_1\ C_1 = \frac{E \times 17.06 \times 70}{180\ E} = 6.63 \text{ ft.};$$

$$E_2\ C_2 = \frac{E \times 21.32 \times 70}{180\ E} = 8.29 \text{ ft.}$$

v_1 and v_2, respectively, are the middle points of $C_0\ B_1$ and $B_1\ B_2$. From each of these points draw a horizontal line. The sloping line, $E_1\ C_0$, intersects the horizontal line drawn through v_1 at F_1. V_1 is on the vertical through C_0. In a similar manner, the sloping line, $E_2\ C_1$, intersects the horizontal line drawn through v_2 at F_2, and V_2 is on the vertical through C_1.

$F_1\ V_1$ and $F_2\ V_2$ are to be considered as vertical forces acting through the centers of gravity of the respective areas to which they belong. Thus $F_1\ V_1$ acts through the center of gravity of the triangle, $C_0\ C_1\ B_1$. $F_2\ V_2$ acts through the center of gravity of the trapezoid, $C_1\ C_2\ B_2\ B_1$. These forces are directly proportional to the areas to which they belong, divided by the respective girder's moment of inertia. It is a matter of prime importance that the exact center of gravity of these trapezoids be computed accurately when they are relatively large, like this one. An old method of doing this graphically is here described, not because engineers may not know it, but because most people easily forget these things when they are out of their line. It is shown in Fig. 14. $A\ C\ D\ B$ is the trapezoid, and $H\ L$ a line that bisects the sides, $A\ B$ and $C\ D$. On $A\ B$, produced, lay off $A\ E = C\ D$, and on $C\ D$, produced, lay off $D\ F = A\ B$, and draw $E\ F$. Then G, at the intersection of $E\ F$ and $H\ L$, is the center of gravity.

Construct the force polygon, $O-1-2-h$, by laying off $O-1 = F_1\ V_1$ and $1-2 = F_2\ V_2$, and assume a pole the perpendicular distance of which from the force line (= J) is here assumed at 30 ft. From this the equilibrium polygon, $O-1'-2'-l$, is constructed in the usual manner. Prolong the end sides to intersect the verticals through the supports at G and L; then $O\ G = \frac{m\ \theta\ G_1}{J}$, and $l\ L = \frac{m\ \theta\ L_1}{J}$. Now, this factor,

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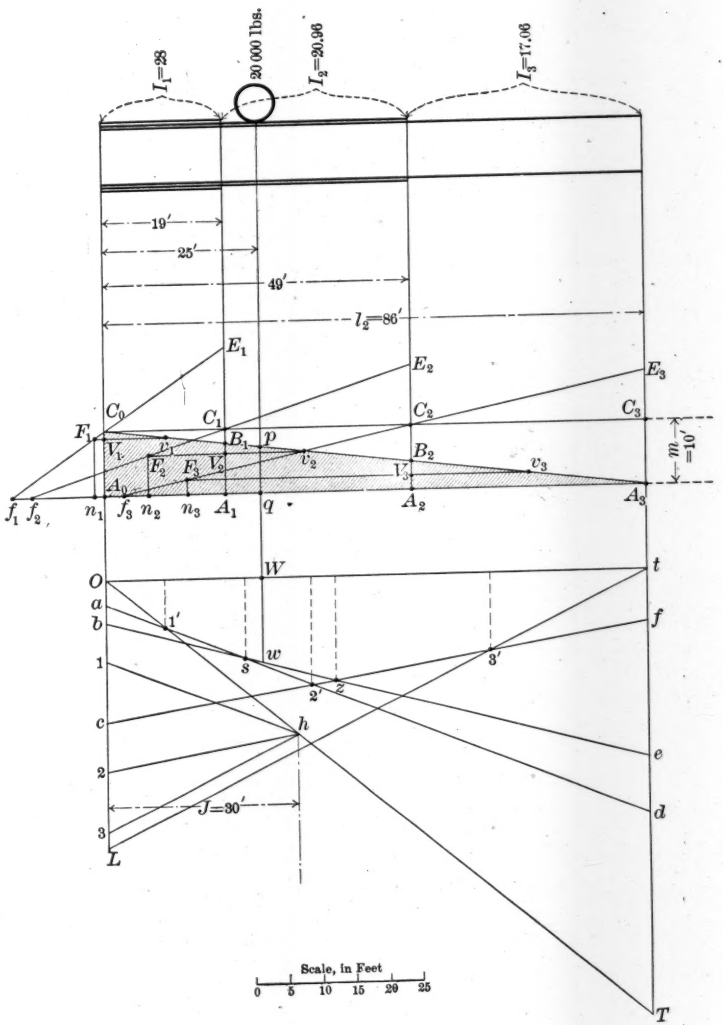

FIG. 13.

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$\frac{m\,\theta}{J} = \frac{10 \times 180\,E}{30} = 60\,E$, and, therefore, if the work done is exactly correct, $O\,G$ should measure 68.088 ft., and $l\,L$ should also be 37.284 ft. Of course, such accuracy is not possible. The figures of these drawings, however, are invariably carried out to several places of decimals, far beyond the ability of the draftsman, even if using a microscope, for thereby the subject is illustrated more accurately.

The next is the calculation of B_1. The vertical through the load, P_1, intersects the closing line of the equilibrium polygon at W, and the triangle, $C_0\ C_2\ B_2$, in a line, $p\ q$. The center of gravity of the trapezoid, $C_1\,p\ q\ B_1$, is in a vertical through s. The center of gravity of the trapezoid, $p\ q\ B_2\ C_2$, is in a vertical through z. Both these points are on the bounding lines of the equilibrium polygon. Draw $s\,z$ and produce it both ways to the verticals through the supports at b and d. It intersects the vertical through W at w.

Then

$$W\,w = 12.9014 \text{ ft.} = \frac{m\,\theta\,B_1}{J\,l_1\,P_1} = \frac{10 \times 180\,E \times B_1}{30 \times 70 \times 40\,000},$$

whence $B_1 = \frac{602\,064}{E}$, as found previously.

Attention is now called to Fig. 13, representing the second span, and from which we are to calculate L_2, T_2, and A_2. The formula for computing $E_1\ C_1$, $E_2\ C_2$, etc., applies, as heretofore given, and we therefore deduce (omitting E):

$$E_1\ C_1 = \frac{28 \times 86}{180} = 13.38 \text{ ft.}$$

$$E_2\ C_2 = \frac{20.96 \times 86}{180} = 10.01 \text{ ft.}$$

$$E_3\ C_3 = \frac{17.06 \times 86}{180} = 8.15 \text{ ft.}$$

$F_1\ V_1$, $F_2\ V_2$, etc., are computed graphically in the same manner as in Fig. 12, and they are used for similar purposes. That is, when necessary, G_2, B_2, and incidentally L_2, are computed from these in the same way as in Fig. 12.

The lines that are used as forces here, however, are not these, but are $f_1\ n_1$, $f_2\ n_2$, etc.

The manner of computing any of these will be explained from one example. Suppose the point, F_2, is found. Produce the sloping line, $E_2\ C_1$, through F_2 until it intersects the bottom line of the parallelogram at f_2. A vertical through F_2 intersects this bottom line at n_2. The lines, $f_1\ n_1$, $f_3\ n_3$, are found in the same manner.

These lines, $f_1\ n_1$, $f_2\ n_2$, etc., are considered as forces, and are directly proportional to the areas to which they belong divided by the respective girder's moment of inertia. These subdivisional areas are contained in the triangle, $C_0\ A_3\ A_0$. $f_1\ n_1$ belongs to the

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trapezoid, $C_0\ B_1\ A_1\ A_0$. $f_2\ n_2$ belongs to the trapezoid, $B_1\ B_2\ A_2\ A_1$. $f_3\ n_3$ belongs to the triangle, $B_2\ A_3\ A_2$. This is a different arrangement from that for $F_1\ V_1$, $F_2\ V_2$, etc., as the subdivisions for these are taken from the other half of the parallelogram. The lines, $f_1\ n_1$, $f_2\ n_2$, etc., are considered as acting through the center of gravity of the areas to which they belong, and are laid off in the force polygon: $f_1\ n_1 = 0\text{–}1$; $f_2\ n_2 = 1\text{–}2$, etc. The pole distance $= J = 30$ ft.

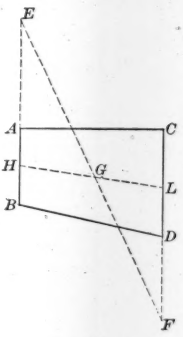

FIG. 14.

The equilibrium polygon, $O - 1' - 2' - 3' - t$, is constructed in the usual manner. Prolong the end sides of this equilibrium polygon to the verticals through the supports, intersecting them at L and T.

Then

$$O\ L = \frac{m\ \theta\ L_2}{J} = 43.458 \text{ ft.}$$

$$T\ t = \frac{m\ \theta\ T_2}{J} = 72\ 678 \text{ ft.}$$

The next is the calculation of A_2. The vertical through the load, P_2, intersects the closing line of the equilibrium polygon at W, and the triangle, $C_0\ A_3\ A_0$, in a line, $p\ q$.

The center of gravity of the trapezoid, $B_1\ p\ q\ A_1$, is in a vertical through s, and the center of gravity of the trapezoid, $p\ B_2\ A_2\ q$, is in a vertical through z. Both these points are on the sides of the equilibrium polygon.

Draw $s\ z$ and produce it to b and ε on the verticals through the supports. It intersects the vertical through W at w. Then,

$$W\ w = \frac{m\ \theta\ A_2}{J\ l_2\ P_2} = \frac{10 \times 180\ E \times A_2}{30 \times 86 \times 20\,000} = 13.9806 \text{ ft.}$$

Hence,

$$A_2 = \frac{400\ 776}{E}, \text{ as previously found.}$$

The graphical computation is now complete, provided no mistake has been made. If the drawing and the tabular calculation disagree, it is well to check one against the other.

Commence with the first span:

$$O\ a = \frac{m\ \theta}{J}\ \gamma_1 = 12.12 \text{ ft.}$$

$$O\ b = \frac{m\ \theta}{J} \sum_o^{a_1} \gamma = 22.296 \text{ ft.}$$

$$c\ L = \frac{m\ \theta}{J}\ \lambda_1 = 22.272 \text{ ft.}$$

$$d\ L = \frac{m\ \epsilon}{J} \sum_o^{a_1} \lambda = 28.98 \text{ ft.}$$

$$F_1\ V_1 = 14.7391 \text{ ft.}$$

$$F_2\ V_2 = 30.4198 \text{ ft.}$$

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Now refer to Fig. 13.

$$O\,a = \frac{m\,\theta}{J}\,\lambda_1 = 3.834 \text{ ft.}$$

$$O\,b = \frac{m\,\theta}{J}\sum_{o}^{a_3}\lambda = 7.098 \text{ ft.}$$

$$O\,c = \frac{m\,\theta}{J}\,(\lambda_1 + \lambda_2) = 23.496 \text{ ft.}$$

$$T\,d = \frac{m\,\theta}{J}\,\tau_1 = 32.382 \text{ ft.}$$

$$T\,\varepsilon = \frac{m\,\theta}{J}\sum_{o}^{a_3}\tau = 41.904 \text{ ft.}$$

$$T\,f = \frac{m\,\theta}{J}\,(\tau_1 + \tau_2) = 64.65 \text{ ft.}$$

$f_1\,n_1 = 12.6344$ ft.; $f_2\,n_2 = 18.1138$ ft.; $f_3\,n_3 = 9.7646$ ft.
$F_1\,V_1 = 1.5689$ " $F_2\,V_2 = 11.8436$ " $F_3\,V_3 = 35.6289$ "
$f_1\,A_0 = 14.2033$ " $f_2\,A_1 = 29.9574$ " $f_3\,A_2 = 45.3935$ "

From the above, considerable checking could be done, but the best method is to check the tables directly, thus:

If v is the number of a section, then

$$f_v\,A_{v-1} = \frac{m\,\theta}{E\,I_r}\,\Delta\,\varepsilon_v$$

To illustrate this, and check the table, let $v = 2$, in Fig. 13; then

$$f_2\,A_1 = 29.9574 \text{ ft.} = \frac{10 \times 180\,E}{E \times 86} \times \Delta\,e_2 \text{ ft.};$$

whence $\Delta\,\varepsilon_2 = 1.4313$, as in the table.

Again,

$$F_v\,V_v = \frac{m\,\theta}{2\,E\,l^2_r}\,\Delta\,\varepsilon^2_v,$$

and, making $v = 2$,

$$F_2\,V_2 = 11.8436 \text{ ft.} = \frac{10 \times 180\,E}{2 \times E \times 7\,396} \times \Delta\,\varepsilon^2_2 \text{ ft.}$$

$\Delta\,\varepsilon^2_2 = 97.3282$, as in the table.

$\Delta\,\varepsilon^3_v$ can easily be checked from the values of γ_v in Fig. 12. In Fig. 13, however, a different plan will have to be adopted. Let $F_v\,V^a$ be called q^1_v and let X_v be the distance of the center of gravity of the subdivisional area from the left support, then

$$q^1_v\,X_v = \frac{m\,\theta}{3\,EI^3_r}\,\Delta\,\varepsilon^3_v.$$

In Fig. 13, let $v = 2$. $F_2\,V_2 = 11.8436$ ft. The area to which it belongs is the trapezoid, $C_1\,C_2\,B_2\,B_1$, and $X_v = 36.2059$ ft.

Then

$$11.8436 \times 36.2059 = \frac{10 \times 180\,E}{3\,E \times 7\,396} \times \Delta\,\varepsilon^3_2$$

Whence $\Delta\,\varepsilon^3_2 = 5\,285.7824$, as in the table.

There is another method of calculating G_1, L_1, L_2, T_2, which must be mentioned.

If the lines that are taken as forces are considered as such, then there must be reactions at the supports due to these forces. Therefore, in Fig. 12, the reaction at the left support $= \frac{m\,\theta}{l_1} L_1 = 15.9786$ ft. The reaction at the right support $= \frac{m\,\theta}{l_1} G_1 = 29.1803$ ft. In Fig. 13, the reaction at the left support $= \frac{m\,\theta}{l_2} T_2 = 25.325$ ft. That at the other support $= \frac{m\,\theta}{l_2} L_2 = 15.193$ ft. The unit of force = the unit of length = 1 ft. These reactions are found in the usual manner by drawing a line from the pole parallel to the closing line of the equilibrium polygon. Mr. Lindenberger.

The reader may notice the ill-conditioned triangles about the parallelogram. These were caused by too large a value of θ, but the latter could not be diminished without increasing the vertical dimensions of the drawing. The proper value of θ, is when $E_1\,C_1$, $E_2\,C_2$, etc., have such values that $E_1\,C_0$, $E_2\,C_1$, etc., slope about at an average angle of 45 degrees. The product, $m \times \theta$, is determined by the smallest length of line that is to be inserted in the force polygon. Finally, $\frac{m\,\theta}{J}$ is determined by the intercepts on the verticals.

A little practice will enable one to assume proper values.

When $l_1 = l_2$, and the girder is symmetrical with reference to its center, the problem is simpler. $G_1 = T_2$; $L_1 = L_2$. Also, if any two loads on these two different spans are at equal distances from the center, then, for these, $A_2 = B_1$. This is a very common case, and one in which only half the girder need be calculated.

It is the experience of everyone that an illustration giving numerical results will do more to clear up difficulties than any amount of analytical formulas. This work, therefore, should have been done long ago, but was not, for several very good reasons. The writer thinks that the most important practical difficulties that would confuse a computer have been pretty thoroughly explained.

C. W. Hudson, M. Am. Soc. C. E. (by letter).—It was not the writer's intention to consider the deformations due to the auxiliary load of unity, in his derivation of Mr. Hudson.

$$\Delta = \int_o^l \frac{M\,m\,d\,x}{E\,I} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3)$$

This expression is obtained by making the internal work,

$$W = \int_o^l \frac{M\,m\,d\,x}{2\,E\,I} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

equal to the external work $W = \frac{\Delta}{2}$ ..... (2)

The consideration of the increased deformations of the fibers, due

Mr. Hudson. to the load of unity, would increase the work of the internal stresses by exactly the same amount that the consideration of the increased deflection due to the load of unity would increase the work of the external force.

This statement appears axiomatic, and therefore needs no proof.

To overcome any seeming obscurity, the foregoing statement is proved by the writer, as follows:

Let Δ_1 = the deflection of the point of application of the force of unity due to its own action;

λ_1 = the change in length of any fiber due to the force of unity; $= \frac{m\,y\,d\,x}{E\,I}$, as was shown for the given loading.

The total change of length of any fiber now

$$= \frac{M\,y\,d\,x}{E\,I} + \frac{m\,y\,d\,x}{E\,I} = (M+m)\,\frac{y\,d\,x}{E\,I},$$

and the internal work throughout the beam,

$$W = \int_o^l \frac{M\,m\,d\,x}{2\,E\,I} + \int_o^l \frac{m^2\,d\,x}{2\,E\,I} \quad \ldots\ldots\ldots\ldots\ldots\ldots (10)$$

The total deflection desired, now $= \Delta + \Delta_1$, and the external work done by the force of unity $= (\Delta + \Delta_1) \times \frac{1}{2}$ ..... (20)

Making Equation (10) = Equation (20), we have

$$\Delta + \Delta_1 = \int_o^l \frac{M\,m\,d\,x}{E\,I} + \int_o^l \frac{m^2\,d\,x}{E\,I};$$

but $\Delta_1 = \int_o^l \frac{m^2\,d\,x}{E\,I}$, therefore $\Delta = \int_o^l \frac{M\,m\,d\,x}{E\,I}$.

That the deflection of any point in a simple beam with either constant or variable moment of inertia can be more easily determined from $\Delta = \int_o^l \frac{M\,m\,d\,x}{E\,I}$, than from $\frac{d\,y^2}{d\,x^2} = \frac{M}{E\,I}$, can very readily be seen. The algebraic work in the solution, in general terms, of comparatively simple problems by means of the second of the foregoing being very laborious, if not impossible.

The writer is sorry that the time at his disposal did not permit of his making a determination of the amount of work done by the shearing stresses on the vertical legs of the flange angles and on the large vertical side-flange plates in the girder of Case II. He feels sure that, for this case, neglecting this work at least balances the omission of the coefficient $\frac{6}{5}$. For the girder of Case I it would have been more accurate to have used this coefficient, and the formula for its investigation would therefore more nearly be

$$\Delta = \int_o^l \frac{M\,m\,d\,x}{E\,I} + 3\int_o^l \frac{S\,s\,d\,x}{E\,A\,w}.$$

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

Paper No. 957.

AN INVESTIGATION OF THE PROPERTIES OF BRICK, UNDER DIFFERENT PHYSICAL CONDITIONS.*

By Sherman M. Turrill, Assoc. Am. Soc. C. E.

With Discussion by Messrs. E. J. McCaustland and Sherman M. Turrill.

This paper is based upon tests conducted in the Civil Engineering Laboratories at Cornell University, during the early part of the year 1901, by the writer, assisted by Professor C. L. Crandall, with suggestions from Professor G. S. Williams.

The methods of procedure, the results of the tests, and the outline of the conclusions are given; and the writer hopes that a complete discussion of the conclusions and a comparison of the work with preceding work in the same line may be brought out.

General Description of the Investigation.

All the brick used in this investigation were common building brick, made at Horseheads, New York, by the Horseheads Brick Company, and were of the usual size (8 x 4 x 2 ins.), but varied, more or less, from these dimensions, being a little less than 4 ins. wide and a little more than 2 ins. thick. In their manufacture a mixture of clay and shale was used, and they were made by the stiff-mud process, under modern methods.

* Presented at the meeting of April 15th, 1903.

The tests conducted were as follows:

Compressive tests of whole brick on end, edge, and flat, and of brick cubes;

Tension tests;

Bending or transverse tests;

Shearing and torsion tests.

The conditions were:

Natural brick;

Brick filled with water;

Reheated brick.

By "natural brick" is meant brick in its natural condition when placed on the market. By "brick filled with water" is meant brick placed under water for a sufficient period to be thoroughly saturated prior to testing, this condition existing until the test was completed. By "reheated brick" is meant brick which has been subjected to a high temperature after its entrance into the commercial world.

In making the tests for compression, the bearings were ground, and, in addition, plaster of Paris and building paper were used. The cubes were, in general, 2 ins. square, two cubes being sawed from one brick. In the tension and torsion tests, the surfaces were not ground, but building paper was used for a bearing. In the shearing and bending tests, the surfaces were not ground, nor was plaster of Paris or paper used on the bearings.

Purpose of the Investigation.

The object in carrying out these tests was to ascertain the weak points of a brick, and, as brick are subjected to compression, tension, bending, shear and torsion, to a greater or less degree, it seemed best to test them under all these conditions and compare the results.

In testing brick under the three conditions, natural, filled with water, and reheated, the writer had in mind brick used in dry places, brick used in wet places, and brick which were to be used again after having passed through a great fire.

In dealing with brick continually under water, without taking into consideration the effect of freezing, one might question their strength as compared with natural brick. In that part of the investigation, special pains were taken to see that the brick were placed under water for at least 1½ months, so that they should be, as nearly as possible, in the actual condition of brick continually under water.

In the case of reheated brick, as, for example, those which have passed through a great conflagration, there is a question as to the advisability of using such brick, and that was one of the reasons for including this condition in the investigation. In order to bring about this condition, as well as to find the weight of the brick when void of moisture, an oven was built, Fig. 1, Plate I, in which the specimens were heated. After the brick were cooled, each one was sounded with a hammer. If it was shaky, it was thrown away; but if it had the ring of a sound brick, it was saved and tested. The question then arose: "Is the hammer test reliable as to the soundness of reheated brick?" The results of these tests show that it is, and that by testing reheated brick with a hammer, about 12% of them were found to be shaky.

PROPERTIES OF THE BRICK TESTED.

Analysis.—The following is an analysis of the brick tested:

Silica	($Si\ O_2$)	64.61 per cent.
Alumina	(Al_2O_3)	20.10 "
Iron oxide	(Fe_2O_3)	6.30 "
Calcium oxide	($Ca\ O$)	6.21 "
Magnesia	($Mg\ O$)	2.50 "
Sulphur trioxide	($S\ O_3$)	trace "
Loss on ignition		0.08 "
Total		99.80 "
Specific Gravity		2.66

Absorption of Water.—In determining the quantity of water absorbed, 500 brick of such varying hardnesses as would represent all grades found in building brick, were first selected. These brick were then placed in the oven shown in Fig. 1. Plate I, and were subjected to a high degree of heat, night and day, for one week. At the end of that time it was found that the moisture in the brick had entirely disappeared. Each brick was then taken out of the oven separately and weighed while it still contained its maximum heat, the fire being kept going until every brick was weighed. The weight of each of the 500 brick in the dry state was ascertained by this means.

These 500 brick were next placed under water for about 6 weeks, until they had absorbed all the moisture they could. Each brick was

then taken out separately and weighed, and thus the weight of each brick saturated with moisture was determined. Before weighing, the surplus water on the surface was removed by means of blotters. This gave the two weights necessary for determining the quantity of moisture absorbed.

Table No. 1 shows the range of absorption found, and is a good guide as to the absorption in common building brick. In this table it is seen that brick No. 63 has the lowest absorption, being only 147.4 gr., which is only 7% of its weight in the dry state; while the highest absorbed 490.7 gr., which is 24.34% of its dry weight. The average results from the 500 brick are also recorded in Table No. 1, and show a larger percentage of absorption than is generally allowed in specifications for common building brick. In specifications, in determining the percentage of absorption to be permitted, the weights of the natural brick and bricks filled with water are generally used. This method would tend to decrease the percentage of absorption, as recorded in Table No. 1, and bring it very close to that specified generally.

TABLE No. 1.—Absorption of Water in Brick.

Brick No.	Weight, in grams, before immersion.	Weight, in grams, after immersion.	Weight, in grams, of water absorbed.	Percentage of water absorbed.	Remarks.
42	1 993.0	2 193.6	200.6	10.07	
63	2 105.8	2 249.7	147.4	7.00	Very hard. Absorbed less water than any one of the 500 brick.
103	2 128.5	2 449.1	320.6	15.06	
109	2 015.8	2 506.5	490.7	24.34	Very soft. Absorbed more water than any one of the 500 brick.
173	1 995.5	2 222.8	227.3	11.39	
190	2 021.2	2 341.8	321.6	15.91	
217	1 997.0	2 315.7	318.7	15.95	
221	2 012.4	2 211.6	199.2	9.89	
225	2 087.0	2 369.4	282.4	13.53	
296	2 135.5	2 502.3	366.8	14.62	
365	2 129.1	2 462.6	333.5	15.66	
400	2 026.5	2 347.8	321.3	15.85	
Averages	2 053.9	2 347.7	294.2	14.19	
Average results from 500 specimens tested	2 030.52	2 351.39	320.87	15.80	

In constructing the oven, a hole, for an ash pit, was dug in the ground, the grate was placed on a level with the ground, and around this was built the oven. No bonding material was used in the casing

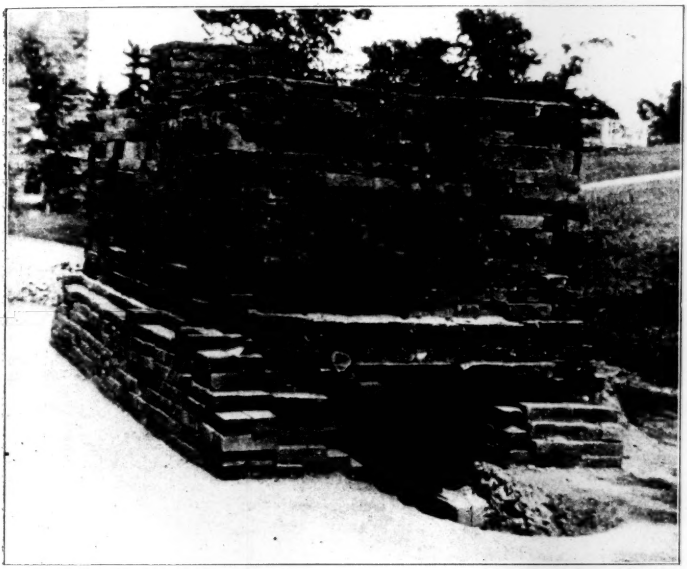


FIG. 1.—BRICK OVEN, WHERE MOISTURE IN BRICK WAS DRIVEN OFF.

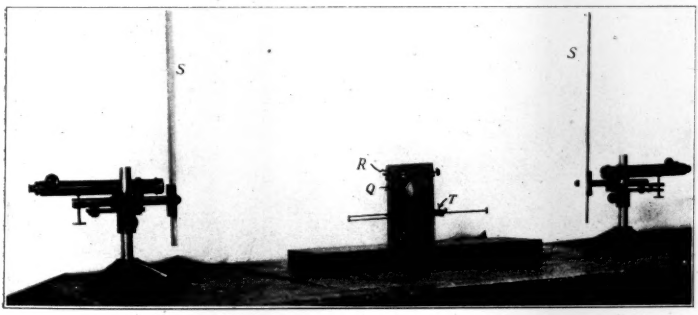


FIG. 2.—APPARATUS USED IN DETERMINING THE ELASTIC PROPERTIES OF BRICK.
R = ROCKER BEARING. S = SCALE.
Q = ROCKER LEVER. T = CLAMP.

of the oven, the bricks being piled so as to break joints properly. In places where beams were required, wrought-iron pipes were used, and the brick placed on them. In order to prevent leakage of heat, as much as possible, the larger openings in the joints were filled with sand, and the roof was covered with a thick layer of the same substance.

Method and Apparatus Used in Determining the Elastic Properties.

All the brick used in determining the elastic properties were tested endwise. The initial load was 100 lbs., and when this load was applied, the initial reading was taken. A total load of 500 lbs. was then applied and a second reading taken. The load was then removed until a total of 100 lbs. was again obtained and a third reading taken. A pressure of 1 000 lbs. was applied and a fourth reading taken. The loading was again reduced to 100 lbs. and a fifth reading taken. This method of loading, unloading and reading was followed until it became evident that it would no longer be safe to allow the apparatus to remain attached to the brick. This part of the apparatus was then taken off, and the brick was tested for compression, endwise.

The foregoing method was followed with each of the bricks, and, for each, four curves were plotted as guides in determining its elastic properties. Two of the curves were plotted from readings taken from one edge, and the other two from the opposite edge of the brick. The plotting of the values on each side, computed from the differences of readings between the initial and the successive 100-lb. readings, gives the curve of permanent set. The two curves, one on each side, plotted from the values derived from the differences of readings between the initial loading and those of 500, 1 000, 1 500 lbs., etc., respectively, give the curve of distortion under the respective loadings. The average of the curves of permanent set for each brick was ascertained by taking the arithmetic mean—that is, the sum of the two values for the corresponding loading divided by two. The same method was followed in ascertaining the averages of the curves of distortion, thus making two average curves for each brick.

The apparatus used is shown in Fig. 2, Plate I, and consists essentially of two sets of parts, those not attached to the brick and those attached to the brick. In the former, for each side of the brick,

there is a standard containing a telescope with vertical and horizontal slow-motion screws, and a scale, S, graduated to centimeters. The latter consists of two rocker-levers, Q; rocker-bearings, R, with a mirror at one end of each, one of each for each side of the brick; and a clamp, T.

Where R and the lower end of Q would naturally bear against the brick, a small brass plate, in each place, was cemented to the brick, giving a brass bearing for these points. The details at R and the lower end of Q are shown in Fig. 1.

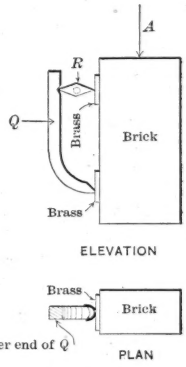


FIG. 1.

Now, as pressure is applied on the end of the brick, in the direction of the arrow, A, as shown in Fig. 1, the inside corner of R moves down, revolving about the edge in the groove of Q as a center. By this means, the mirror at the end of R moves through the same angle as R, thus giving different readings on the scale, S. The clamp, T, was used to hold Q and R in position.

When a total load of 100 lbs. was applied, the investigator looked through the telescope at the mirror and read the scale, in centimeters. On applying a heavier load on the brick, the mirror revolved, and a reading higher up the scale was obtained. The question then resolved itself into this form: How to find, from the differences of readings, the distortions under the respective loadings? Being able to find the differences of readings on the scale, then, by placing the scale, S, at a definite distance from the mirror, a constant was determined, which, when divided into the differences of scale readings, would give the distortions per unit of length for corresponding differences of loadings. This quantity is called the constant of the machine.

The determination of the constant for the apparatus is worked out as follows:

Length of $Q = 15$ cm.

Horizontal distance between bearings of $R = 0.44958$ cm.

Let the difference of readings (which means the difference between the initial reading and any other reading taken thereafter) be denoted by a; the distance from the mirror to the scale is denoted by z. After

a little computing, it was found that, with a constant of 6 000, a convenient value for z was obtained.

Let the total amount of distortion be denoted by x, then the distortion per unit of length is $= \frac{x}{15}$.

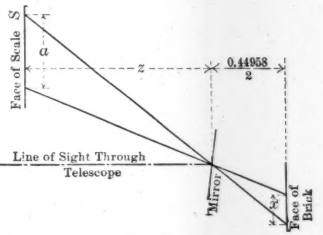


FIG. 2.

In the similar triangles of Fig. 2:

$$a : x = z : \frac{0.44958}{2}$$

$$\text{Therefore} \qquad x = \frac{\frac{0.44958\ a}{2}}{z} = \frac{a}{\frac{2\,z}{0.44958}}$$

$$\text{Therefore} \qquad x = \frac{\text{Difference of scale reading.}}{\frac{\text{2 distance of scale from mirror.}}{0.44958}}$$

$$\text{Hence} \qquad \frac{x}{15} = \frac{\text{Difference of scale reading.}}{\frac{15 \times \text{2 distance of scale from mirror.}}{0.44958}}$$

$$= \text{distortion per unit of length; and is applicable to any system of units.}$$

$$\text{Let} \qquad 6\,000 = \frac{15 \times \text{2 distance of scale from mirror.}}{0.44958}$$

$$\text{Therefore } z, \text{ distance of scale from mirror, } = \frac{6\,000 \times 0.44958}{2 \times 15}$$

$$= 89.916 \text{ cm.} = 34.4 \text{ ins.} = 2.95 \text{ ft.}$$

$$\text{Therefore} \qquad \frac{x}{15} = \frac{\text{Difference of scale reading.}}{6\,000}$$

Then, by placing the scale, S, 2.95 ft. from the mirror, the distortion per unit of length $\left(\frac{x}{15}\right)$ was obtained at a glance.

By this method the quantities used in plotting the elastic curves were obtained.

Compressive Tests of Brick.

This set was tested in the Riehlé, 400 000-lb. testing machine, and consists of brick on edge, on end, and flat, and 2-in. cubes, for natural, filled-with-water, and reheated brick. The brick tested on end were first tested for elastic properties, and, after the mirror micrometers had been removed, were tested for compression.

The photographs on Plates III, IV and V show that in all cases the fractures are, in general, double-coned in outline, and that the apices of the cones meet in the middle. It will also be seen that the angle of the fracture with the bed-plane changes somewhat according to the way the bricks were tested, and varies more or less from 60 degrees.

In testing any material under compression it is claimed that a more accurate test is obtained by using cubes instead of rectangular blocks, the theory being that if tested on the flat a considerable percentage of the stress necessary for crushing is used up in friction, which takes place when the outside portion of the brick is pushed away to give room for the interior to crush, thus causing a greater stress than is necessary for crushing. In the case of testing on edge and on end it is advanced that bending, as in a column, is introduced to some extent. In the case of the cube there is no loss of stress due to friction on the bed-plate or head-block, and no tendency to bending, as in a column; and these, alone, are good reasons for selecting the cube as the ideal form of test piece.

By comparing the averages shown in Tables Nos. 2 to 8, it will be seen that in every case brick on the flat are the strongest, those on edge next to the strongest, those on end next to the weakest, while the cubes are the weakest. It will also be seen that in all cases reheated brick are the strongest, while brick filled with water are the weakest in the end and edge tests, and that natural brick are weakest in the flat and cube tests. Taking the natural cube as a standard test piece, the percentages of increase have been computed as follows:

Cube of brick; natural	1.0
Cube of brick; filled with water	2.2
Cube of brick; reheated	4.4
Natural brick; on end	15.2
Natural brick; on edge	27.1
Natural brick; flat	79.3
Brick filled with water; on end	4.1
Brick filled with water; on edge	22.0
Brick filled with water; flat	104.8
Reheated brick; on end	28.9
Reheated brick; on edge	49.3
Reheated brick; flat	121.7

TABLE No. 2.—Compressive Tests of Brick on End, for Natural, Filled-with-Water, and Reheated Brick.

Numbers.			Area of section, in square inches.			Ultimate load, in pounds per square inch.			Equivalent ultimate load per unit of height, in pounds per square inch.		
Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.
97	1	14	8.53	8.66	8.89	3 763	2 092	3 063	30 102	17 391	24 699
98	2	25	8.91	8.77	8.70	1 234	3 193	2 169	9 952	15 945	17 348
99	3	26	8.54	8.91	8.89	2 742	1 603	2 717	22 536	13 120	21 990
100	4	53	8.70	8.65	8.77	2 826	2 281	2 509	22 698	18 393	20 229
101	5	73	8.65	8.72	8.84	2 952	1 977	3 446	23 893	16 004	28 000
102	6	91	8.70	8.53	8.77	2 733	3 153	1 908	22 035	24 436	15 383
103	7	94	8.72	8.58	8.96	1 663	2 539	2 918	13 357	20 074	23 711
104	8	100	8.84	8.84	8.51	2 602	2 553	2 343	21 063	20 825	18 962
105	9	104	8.15	8.86	8.67	2 101	2 289	1 737	16 874	19 028	13 630
106	10	171	8.53	8.72	8.72	3 339	2 523	2 905	23 802	21 527	22 879
107	11	176	8.72	8.91	8.30	2 606	1 829	3 836	21 498	15 059	31 051
108	12	185	8.22	8.54	8.58	2 976	1 802	4 896	23 808	14 865	39 169
Averages.....			8.60	8.72	8.72	2 628	2 320	2 871	20 986	18 889	23 088

TABLE No. 3.—Compressive Tests of Brick on Edge, for Natural, Filled-with-Water, and Reheated Brick.

Numbers.			Area of section, in square inches.			Ultimate load, in pounds per square inch.			Equivalent ultimate load per unit of height, in pounds per square inch.		
Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.
109	13	194	19.30	17.45	18.35	2 137	2 880	3 521	8 146	10 530	13 427
110	14	283	17.96	18.93	18.00	3 099	2 226	4 278	11 711	8 348	13 984
111	15	285	18.18	18.34	18.21	3 021	2 284	2 550	11 610	8 635	9 404
112	16	412	18.32	18.28	18.35	3 199	3 165	2 722	12 096	11 969	10 462
113	17	416	17.96	18.68	18.14	2 878	2 594	3 438	10 614	9 728	13 215
114	18	426	18.39	18.72	18.28	2 055	2 445	3 253	7 771	9 245	12 098
115	19	430	18.61	18.14	17.87	3 050	2 567	2 502	11 628	10 507	9 617
116	20	459	18.03	18.72	18.28	2 621	2 291	3 605	9 829	8 590	13 405
117	21	465	18.03	18.00	18.39	2 949	2 707	3 931	11 150	10 150	14 864
118	22	467	17.77	18.75	18.39	1 897	2 720	4 043	7 290	8 840	15 540
119	23	468	17.75	18.00	18.46	3 913	4 104	2 986	14 674	15 007	11 479
120	24	479	18.21	18.57	19.19	3 164	2 623	3 087	14 459	9 919	11 867
Averages.....			18.21	18.38	18.24	2 832	2 717	3 326	10 748	10 122	12 447

TABLE No. 4.—Compressive Tests of Brick on Flat, for Natural, Filled-with-Water, and Reheated Brick.

Numbers.			Area of section, in square inches.			Ultimate load, in pounds per square inch.			Equivalent ultimate load per unit of height, in pounds per square inch.		
Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.
121	25	128	31.23	30.99	31.37	4 183	5 803	5 256	8 758	12 707	11 662
122	26	160	31.35	31.35	29.77	4 310	4 783	5 783	9 294	10 049	12 651
123	27	161	31.35	31.23	31.11	5 463	4 923	5 831	12 122	10 462	12 572
124	28	181	31.48	31.35	31.11	2 791	4 527	5 307	6 104	8 771	11 775
125	29	189	31.33	31.36	31.36	3 539	3 259	3 541	7 851	6 925	7 745
126	30	279	32.87	31.23	31.74	2 665	5 613	3 862	5 650	11 402	8 327
127	31	346	31.23	31.48	31.62	4 525	4 583	4 419	9 898	10 024	9 804
128	32	411	31.48	31.10	32.12	3 941	5 303	3 975	8 745	11 269	9 069
129	33	431	31.97	31.95	30.98	3 623	4 005	4 482	8 039	8 510	10 084
130	34	432	30.35	31.73	31.86	3 999	3 623	5 792	8 625	8 039	12 670
131	35	442	31.73	31.36	31.24	4 272	5 089	6 520	9 078	11 021	14 059
132	36	487	31.11	31.21	31.48	4 625	3 097	4 500	9 973	6 774	9 563
Averages.....			31.45	31.36	31.31	3 995	4 562	4 939	8 678	9 663	10 892

TABLE No. 5.—Compressive Tests of Natural Brick.

Numbers.			Area of section, in square inches.			Ultimate load, in pounds per square inch.			Equivalent ultimate load per unit of height, in pounds per square inch.		
End.	Edge.	Flat.	End.	Edge.	Flat.	End.	Edge.	Flat.	End.	Edge.	Flat.
97	109	121	8.53	19.30	31.23	3 763	2 137	4 183	30 102	8 146	8 758
98	110	122	8.91	17.96	31.35	1 234	3 099	4 310	9 952	11 711	9 294
99	111	123	8.54	18.18	31.35	2 742	3 021	5 463	22 536	11 610	12 122
100	112	124	8.70	18.32	31.48	2 826	3 199	2 791	22 698	12 096	6 104
101	113	125	8.65	17.96	31.33	2 952	2 878	3 539	23 893	10 614	7 851
102	114	126	8.70	18.39	32.87	2 733	2 055	2 665	22 035	7 771	5 650
103	115	127	8.72	18.61	31.23	1 663	3 050	4 525	13 357	11 628	9 898
104	116	128	8.84	18.03	31.48	2 602	2 621	3 941	21 063	9 829	8 745
105	117	129	8.15	18.02	31.97	2 101	2 949	3 623	16 874	11 150	8 039
106	118	130	8.53	17.77	30.35	3 339	1 897	3 999	53 802	7 290	8 625
107	119	131	8.72	17.75	31.73	2 606	3 913	4 272	21 498	14 674	9 078
108	120	132	8.22	18.21	31.11	2 976	3 164	4 625	23 808	12 459	9 973
Averages.....			8.60	18.21	31.45	2 628	2 832	3 995	20 986	10 748	8 678

TABLE No. 6.—Compressive Tests of Brick Filled with Water.

Numbers.			Area of section, in square inches.			Ultimate load, in pounds per square inch.			Equivalent ultimate load per unit of height, in pounds per square inch.		
End.	Edge.	Flat.	End.	Edge.	Flat.	End.	Edge.	Flat.	End.	Edge.	Flat.
1	13	25	8.60	17.45	30.99	2 092	2 880	5 893	17 391	10 530	12 707
2	14	26	8.77	18.93	31.85	3 193	2 226	4 783	25 945	8 348	10 049
3	15	27	8.91	18.34	31.23	1 603	2 284	4 923	13 120	8 635	10 462
4	16	28	8.65	18.28	31.35	2 281	3 165	4 527	18 393	11 969	8 771
5	17	29	8.72	18.68	31.36	1 977	2 594	3 359	16 004	9 728	6 925
6	18	30	8.53	18.72	31.23	3 153	2 445	5 613	24 436	9 245	11 402
7	19	31	8.58	18.14	31.48	2 539	2 567	4 583	20 074	10 507	10 024
8	20	32	8.84	18.72	31.10	2 553	2 291	5 305	20 825	8 590	11 269
9	21	33	8.86	18.00	31 95	2 289	2 707	4 005	19 028	10 150	8 510
10	22	34	8.72	18.75	31.73	2 523	2 720	3 624	21 527	8 840	8 089
11	23	35	8.91	18.00	31.36	1 829	4 104	5 039	15 059	15 007	11 021
12	24	36	8.54	18.57	31.21	1 802	2 623	3 097	14 865	9 919	6 774
Averages.....			8.78	18.38	31.35	2 320	2 717	4 562	18 889	10 122	9 663

TABLE No. 7.—Compressive Tests of Reheated Brick.

Numbers.			Area of section, in square inches.			Ultimate load, in pounds per square inch.			Equivalent ultimate load per unit of height, in pounds per square inch.		
End.	Edge.	Flat.	End.	Edge.	Flat.	End.	Edge.	Flat.	End.	Edge.	Flat.
14	194	128	8.89	18.35	31.37	3 063	3 521	5 256	24 699	13 427	11 662
25	283	160	8.70	18.00	29.77	2 169	4 278	5 783	17 348	13 984	12 651
26	285	161	8.89	18.21	31.11	2 717	2 550	5 831	21 990	9 404	12 572
53	412	181	8.77	18.35	31.11	2 509	2 722	5 307	20 229	10 462	11 775
73	416	189	8.84	18.14	31.36	3 446	3 438	3 541	28 000	13 215	7 745
91	426	279	8.77	18.28	31.74	1 908	3 253	3 862	15 383	12 098	8 327
94	430	346	8.96	17.87	31.62	2 918	2 502	4 419	23 711	9 617	9 804
100	459	411	8.51	18.28	32.12	2 343	3 605	3 975	18 962	13 405	9 069
104	465	431	8.67	18.39	30.98	1 737	3 931	4 482	13 630	14 864	10 084
171	467	432	8.72	18.39	31.86	2 905	4 043	5 792	22 879	15 540	12 670
176	468	442	8.30	18.46	31.24	3 836	2 986	6 520	31 051	11 479	14 059
185	479	487	8.58	19.19	31.48	4 896	3 087	4 500	39 169	11 867	9 563
Averages.....			8.72	18.24	31.31	2 871	3 326	4 939	23 088	12 447	10 832

TABLE No. 8.—Compressive Tests of Two-Inch Cubes, for Natural, Filled-with-Water, and Reheated Brick Cubes.

Numbers.			Area of section, in square inches.			Ultimate load, in pounds per square inch.			Equivalent ultimate load per unit of height, in pounds per square inch.		
Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.
*1	*13	95*a*	4.00	4.13	3.88	1 750	1 467	2 328	3 500	2 934	4 556
2	14	95*b*	4.13	4.00	4.13	3 985	3 500	2 179	7 970	7 000	4 358
3	15	152*a*	4.00	4.19	3.94	2 830	2 549	2 208	7 429	6 691	4 278
4	16	152*b*	4.13	4.06	3.94	2 864	2 628	2 292	5 728	5 256	4 584
5	17	228*a*	4.18	4.19	4.00	3 403	3 819	2 540	8 933	7 757	5 080
	18*a*	.....	....	4.13	....	.....	1 640	.....	.....	3 331	.....
6	18*b*	228*b*	4.19	4.06	4.06	1 715	1 844	2 709	4 502	3 688	5 518
7	19	347*a*	4.00	3.94	4.06	1 351	1 294	2 345	2 744	2 588	4 690
8	20	347*b*	4.06	4.13	4.06	1 106	1 094	2 841	2 247	2 188	5 682
9	21	421*a*	4.38	4.13	4.00	1 575	1 398	2 106	3 150	2 796	4 278
10	22	421*b*	4.32	4.13	3.94	2 609	2 838	2 142	6 849	5 753	4 284
11	23	447*a*	4.19	4.13	3.88	1 515	1 617	1 885	3 159	3 234	3 716
12	24	447*b*	4.00	4.13	3.94	2 035	3 234	2 338	4 070	8 489	4 530
Averages.....			4.13	4.10	3.99	2 228	2 273	2 326	5 023	4 873	4 638

* Nos. 1 and 13 were taken from the same brick, Nos. 2 and 14 from another brick, and the same order was followed for all the natural, and filled-with-water cubes, excepting in the cases of Nos. 6, 18*a* and 18*b*, in which cases three cubes were cut out of one brick.

A large hole was found in No. 18*a*, and though its test is recorded in this table, it is not included in the averages. No. 18*b* was cut from the same brick, and its test is included in the averages.

Taking the filled-with-water cube and the reheated cube as standards, the following percentages of increase have been computed: 2.1, 17.3, 100.7; 23.4, 43.0, 111.9 for end, edge, and flat, of filled-with-water and reheated brick, respectively. In Tables Nos. 2 to 8 the equivalent ultimate load per unit of height is given in pounds per square inch. This was obtained by multiplying the ultimate load, in pounds per square inch, by the height of the test piece. The object of ascertaining this was to see if some relation could be shown between the strength of a standard test piece (for example, a cube) and any other test piece, when the ratios of the areas to the heights are given.

In regard to the elastic properties, the averages of the results are shown by means of elastic curves, as seen in Figs. 3 and 4 and Plate II. There are two methods of averaging these curves, the results of one being shown in Figs. 3 and 4, and the other on Plate II, and

each method is divided into two sets, *viz.*, curves of compression and curves of permanent set, for natural, filled-with-water, and reheated brick. In the former set the curves are plotted from values obtained while the successive total loads of 100, 500, 1 000, 1 500 lbs., etc., were being applied, and in the latter case while the successive 100-lb. loadings were being applied. In these curves, the initial loading was taken as 100 lbs., which was 11 lbs. per square inch, more or less, instead of zero. The object in doing this was to be sure that the head-block was not removed from the test piece after it was once placed, thus securing the same bearing, which would not have always been the case had zero been used for the initial loading.

All the elastic curves, marked *M* to *T*, *G* to *L* and *A* to *F*, inclusive, are averages of natural, filled-with-water, and reheated brick, respectively. The method of averaging, in the first set, Figs. 3 and 4, taking the natural brick as an example, is as follows:

The total loadings reached by the several bricks, before the rocker bearing and rocker lever were removed, were as follows:

Brick No. 97, " " 101, " " 104, " " 106	18 000 lbs.
" " 108	17 500 "
" " 107	17 000 "
" " 102	16 000 "
" " 99, " " 100	15 000 "
" " 103	14 500 "
" " 105	11 000 "
" " 98	10 000 "

Bricks Nos. 97, 101, 104 and 106 were averaged together up to 18 000 lbs. loading, making an average of four bricks, as shown in Curve *T*. Brick No. 108, together with Bricks Nos. 97, 101, 104 and 106, were averaged up to 17 500 lbs., making an average of five bricks for 17 500 lbs., as shown in Curve *S*. Brick No. 107, together with Bricks Nos. 97, 101, 104, 106 and 108, were averaged up to 17 000 lbs., making an average of six bricks, as shown in Curve *R*. Brick No. 102, together with Bricks Nos. 97, 101, 104, 106, 107 and 108, were averaged

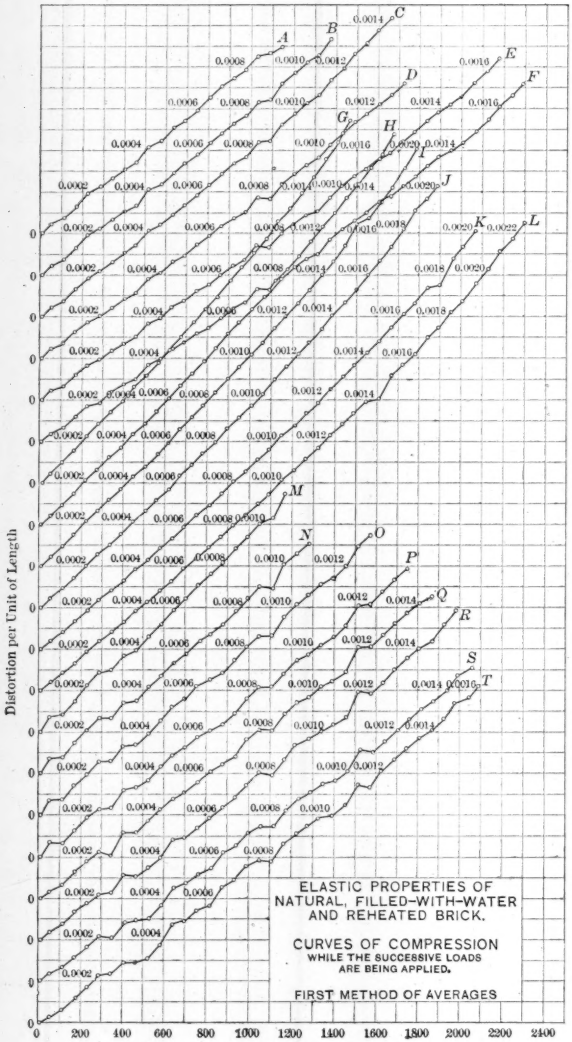
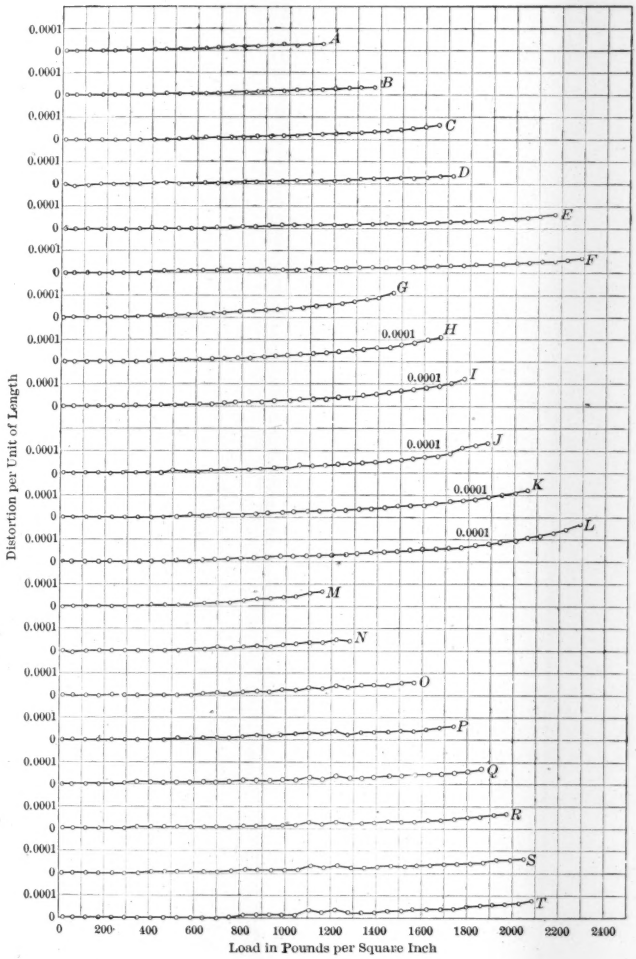

ELASTIC PROPERTIES OF
NATURAL, FILLED-WITH-WATER
AND REHEATED BRICK.
CURVES OF COMPRESSION
WHILE THE SUCCESSIVE LOADS
ARE BEING APPLIED.
FIRST METHOD OF AVERAGES
Distortion per Unit of Length
Load, in Pounds per Square Inch
0
200
400
600
800
1000
1200
1400
1600
1800
2000
2200
2400
A
B
C
D
E
F
G
H
I
J
K
L
M
N
O
P
Q
R
S
T

FIG. 3.



ELASTIC PROPERTIES OF
NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.

CURVES OF PERMANENT SET
AFTER THE SUCCESSIVE LOADS HAVE BEEN APPLIED AND THEN REMOVED

FIRST METHOD OF AVERAGES

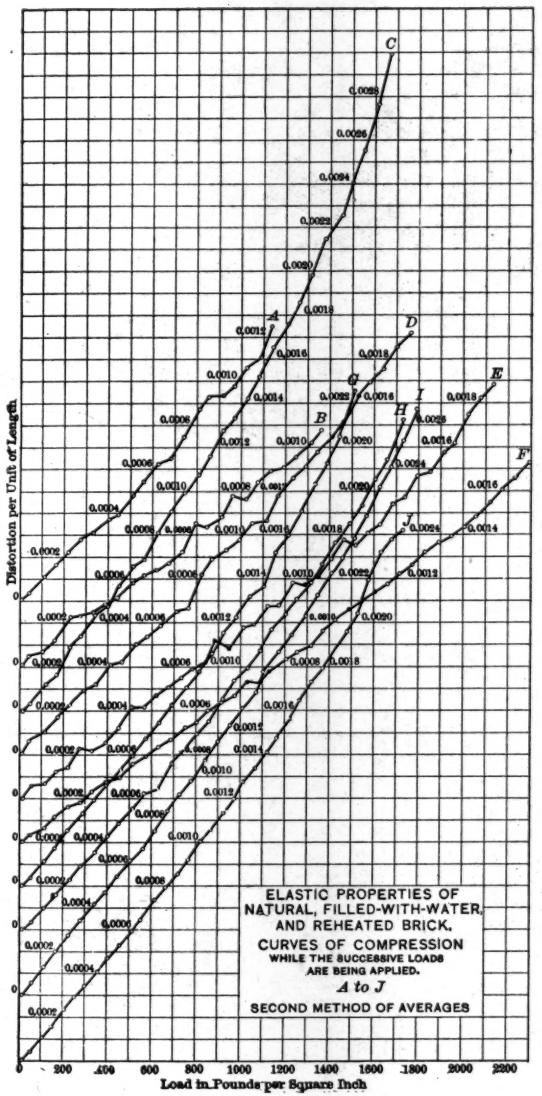
FIG. 4.

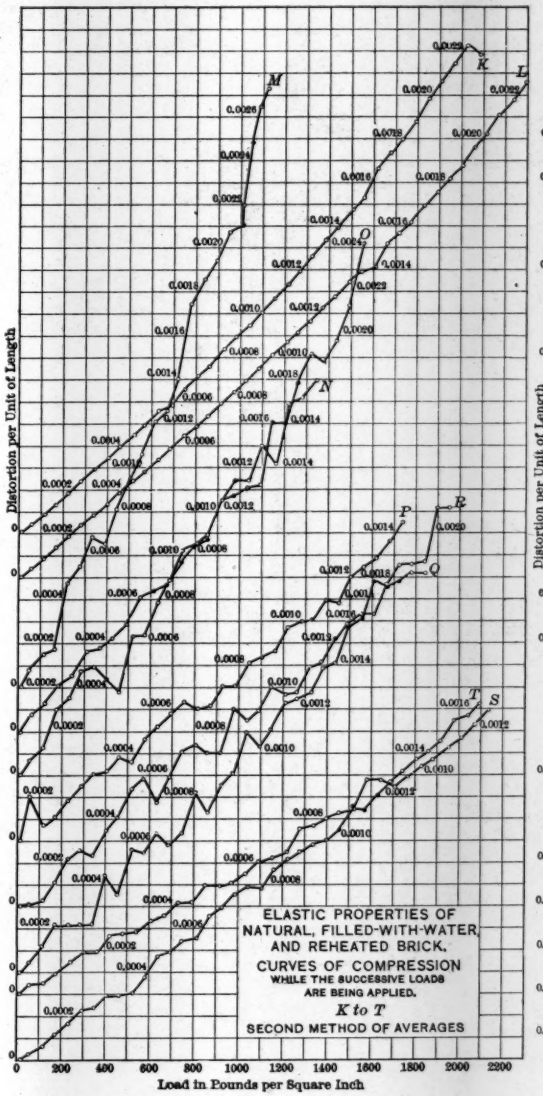
together up to 16 000 lbs., making an average of seven bricks, as shown in Curve *Q*. This method of averaging was followed for the natural, reheated, and filled-with-water brick, each having its own independent averages.

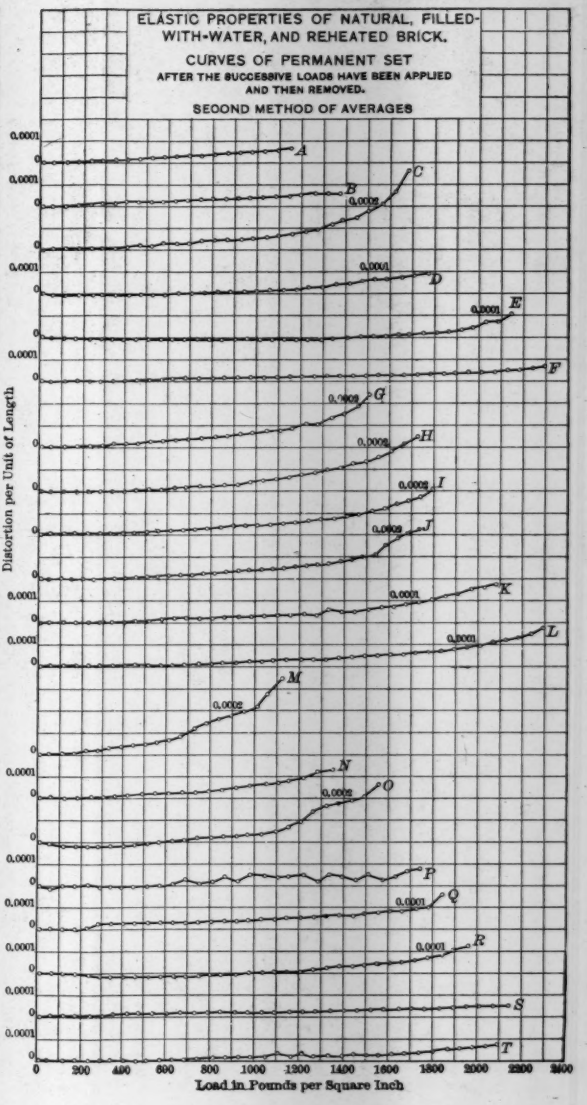
In Figs. 3 and 4 the bricks included in the averages for the several curves are as follows:

Curve.	Numbers of Bricks.
A	Nos. 14, 53, 73, 94, 171, 176 and 185.
B	All bricks in *A*, and No. 26.
C	All bricks in *A*, and Nos. 26 and 100.
D	All bricks in *C*, and No. 104.
E	All bricks in *C*, and Nos. 91 and 104.
F	All bricks in *E*, and No. 25.
G	Nos. 2, 7, 8 and 10.
H	Nos. 2, 4, 7, 8 and 10.
I	Nos. 2, 4, 6, 7, 8, 9 and 10.
J	Nos. 2, 4, 6 and 7 to 11.
K	Nos. 2, 4 and 5 to 11.
L	Nos. 1 to 12.
M	Nos. 97, 101, 104 and 106.
N	Nos. 97, 101, 104, 106 and 108.
O	Nos. 97, 101, 104 and 106 to 108.
P	Nos. 97, 101, 102, 104, and 106 to 108.
Q	Nos. 97, 99 to 102, 104 and 106 to 108.
R	Nos. 97, 99 to 104 and 106 to 108.
S	Nos. 97 and 99 to 108.
T	Nos. 97 to 108.

In the second method, Plate II, only those brick which were loaded up to the same point before removing the mirror micrometers are averaged in one curve. Those bricks (Nos. 97, 101, 104 and 106), carrying a loading of 18 000 lbs. before the removal of the mirror micrometers, are represented by Curve *T*; Curve *S* is the average of all bricks with a loading of 17 500 lbs., which is a single brick, No. 108; Curve *R* is made up of the average of all bricks with a load of 17 000 lbs., and consists of only one brick, No. 107; Curve *Q* is made up of all bricks with a load of 16 000 lbs., and is a single brick, No. 102; Curve *P* is made up of all bricks with a load of 15 000 lbs., and


Distortion per Unit of Length.
Load in Pounds per Square Inch
ELASTIC PROPERTIES OF NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.
CURVES OF COMPRESSION
WHILE THE SUCCESSIVE LOADS ARE BEING APPLIED.
A to J
SECOND METHOD OF AVERAGES


Distortion per Unit of Length
Load in Pounds per Square Inch
ELASTIC PROPERTIES OF NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.
CURVES OF COMPRESSION
WHILE THE SUCCESSIVE LOADS ARE BEING APPLIED.
K to T
SECOND METHOD OF AVERAGES


ELASTIC PROPERTIES OF NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.
CURVES OF PERMANENT SET
AFTER THE SUCCESSIVE LOADS HAVE BEEN APPLIED AND THEN REMOVED.
SECOND METHOD OF AVERAGES
Distortion per Unit of Length
Load in Pounds per Square Inch

consists of the average of two bricks, Nos. 99 and 100. This method of averaging was followed for the natural, reheated and filled-with-water brick, each having its own independent averages.

On Plate II the bricks included in the averages for the several curves are as follows:

Curve.	Number of Bricks.
A	No. 25.
B	No. 91.
C	No. 104.
D	No. 100.
E	No. 26.
F	Nos. 73, 14, 176, 94, 185, 171 and 53.
G	Nos. 1, 3 and 12.
H	No. 5.
I	No. 11.
J	Nos. 6 and 9.
K	No. 4.
L	Nos. 2, 7, 8 and 10.
M	No. 98.
N	No. 105.
O	No. 103.
P	Nos. 99 and 100.
Q	No. 102.
R	No. 107.
S	No. 108.
T	Nos. 97, 101, 104 and 106.

The apparatus used in this test was very sensitive, so sensitive that even the vibration of the scale beam gave a slightly different reading. After taking the different readings, the differences of readings were computed. The values of the ordinates for the curves of compression were taken as the differences between the initial readings and the successive readings at the loadings of 500, 1 000, 1 500 lbs., etc., while the differences of readings for the curves for permanent set mean the differences between the initial readings and the successive 100-lb. readings, taken after the successive loads of 500, 1 000, 15 000 lbs., etc., had been applied.

By measuring an ordinate on a curve of permanent set and reading the value on the load line, one is able to tell at a glance between what

two loadings, on the curve of compression, the 100-lb. reading was taken. For example, suppose there is a certain amount of distortion, measured by the length of an ordinate for a 100-lb. loading, located on the load line at 427 lbs. per square inch. This means that after a load of 427 lbs. per square inch had been applied for the curve of compression, a certain amount of permanent set, measured on that ordinate, took place under the following 100-lb. loading.

As a brick is made up of a granular substance, more or less irregularity in the elastic curves might be expected, and that they would be likely to be more irregular in natural brick, where the granular particles are dry and are thus unable to change their relative positions readily on account of friction, than in the case of brick filled with water, where the granular parts are wet and slippery, thus reducing friction to a minimum and allowing the particles to adjust themselves more freely under the respective loadings; or reheated brick, where the particles have become more solidified, and are now more of a solid mass and less granular, causing the brick to increase in stiffness and its particles to change their relative positions more uniformly. By examining the elastic curves, both for compression and permanent set, it will be seen that this statement is well substantiated; the curves for the natural brick being the most irregular, those for brick filled with water the most regular but the weakest in stiffness, while those for reheated brick show the greatest stiffness.

In testing any granular material, the theory is that these particles slide on one another, and change their relative positions under different loadings. Under a certain loading the particles occupy certain positions, but by applying a heavier loading they change their relative positions, causing, for example, the brick to shorten. Now, by releasing this loading, the particles adjust themselves to different positions. Upon applying a still heavier load it is found, that the brick is not distorted as much as under a less loading, showing that the particles, previous to this loading, had taken a firmer position, and that the irregularities of the curve are due to the greater or less compactness assumed by the particles from time to time.

Modulus of Elasticity.

The modulus of elasticity for compression was found for the initial loading and for every 500 lbs. of loading up to 10 000 lbs. for each

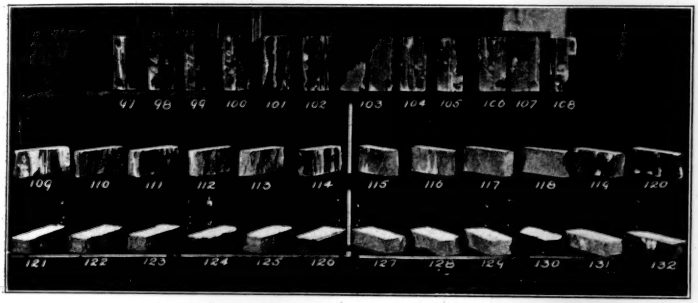


FIG. 1.—COMPRESSIVE TEST OF NATURAL BRICK.
NOS. 97 TO 108, COMPRESSED ON END.
NOS. 109 TO 120, COMPRESSED ON EDGE.
NOS. 121 TO 132, COMPRESSED ON FLAT.

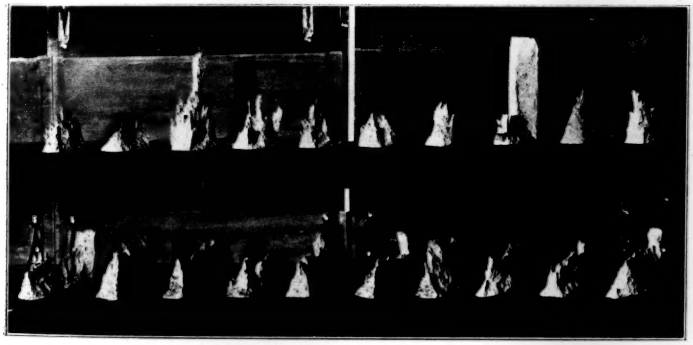


FIG. 2.—COMPRESSIVE TEST OF NATURAL BRICK.
ALL LOOSE PIECES HAVE BEEN REMOVED AFTER CRUSHING.
UPPER ROW, BRICK ON END.
LOWER ROW, BRICK ON EDGE.

brick, thus giving twenty moduli for each brick. The average of these twenty moduli, which run very uniformly in nearly every brick, gave the average for the brick, as recorded in Table No. 9. The average for the twelve brick of each set is also recorded. The lowest modulus obtained in any brick was 500 000 lbs. per square inch, and the highest 2 500 000 lbs. per square inch. It will be seen from Table No. 9 that reheated brick have the highest moduli, while brick filled with water have the lowest.

TABLE No. 9.—MODULI FOR COMPRESSION, FOR NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.

NUMBERS.			MODULI OF ELASTICITY, IN POUNDS PER SQUARE INCH.		
Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.
97	1	14	1 804 000	1 011 000	1 240 000
98	2	25	540 000	1 350 000	1 056 000
99	3	26	1 598 000	749 000	1 336 000
100	4	58	1 284 000	1 205 000	1 631 000
101	5	73	1 542 000	950 000	1 250 000
102	6	91	1 610 000	827 000	1 323 000
103	7	94	796 000	1 355 000	1 334 000
104	8	100	1 173 000	1 122 000	1 034 000
105	9	104	924 000	904 000	840 000
106	10	171	1 446 000	1 136 000	1 278 000
107	11	176	1 102 000	806 000	2 200 000
108	12	185	1 815 000	894 000	1 823 000
Averages.....			1 305 000	1 026 000	1 362 000

In computing the moduli, use was made of Hook's Law; that is, the distortion is proportional to the stress, within the elastic limit. Hence the moduli were obtained by dividing the stress by the corresponding distortion per unit of length.

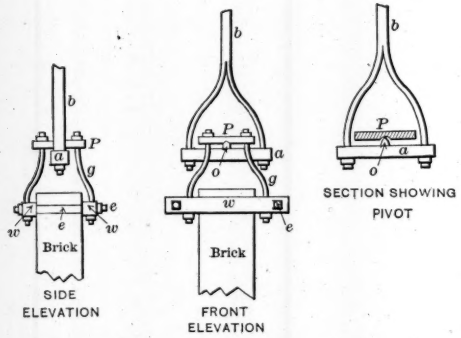
The writer would have liked to determine also the moduli for shear, torsion, bending and tension, in addition to that for compression, if the distortions had been of sufficient magnitude to be measured.

TENSION TESTS OF BRICK.

In making the tension tests, the Riehlé, 400 000-lb. testing machine was used, the apparatus consisting essentially of two clamps, one for each end of the brick, on a pivot bearing, as shown in Fig. 5.

The rod, *b*, was attached to the head-block of the testing machine and was allowed considerable lateral motion, so as to adjust itself to a direct pull on the brick during testing. The square bar, *a*, was

bolted to the spread portion of the rod, *b*, and contained at its middle a pivot, *o*, upon which the plate, *P*, and the lower part of the clamp had its bearing. The plate, *P*, rests upon *o* with considerable range of movement, for the sake of direct pull during testing, as shown by the cone-shaped impression in Fig. 5. The bent rods, *g*, were not attached



CLAMPS USED IN TENSION TESTS.

FIG. 5.

TABLE No. 10.—TENSION TESTS OF NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.

NUMBERS.			AREA OF SECTION, IN SQUARE INCHES.			ULTIMATE LOAD, IN POUNDS PER SQUARE INCH.		
Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.
1	25	96	8.89	8.65	8.51	269	201	411
2	26	133	8.53	8.84	8.58	292	189	266
3	27	142	8.77	8.84	8.51	205	219	182
4	28	155	8.72	8.67	8.58	123	218	163
5	29	191	9.11	8.96	8.84	149	112	169
6	30	215	9.08	8.82	8.89	127	204	149
7	31	231	8.82	8.84	8.44	263	242	402
8	32	422	8.67	8.77	8.55	111	186	216
9	33	428	8.65	8.96	8.60	282	234	159
10	34	459	8.89	8.65	8.70	328	278	161
11	35	460	8.65	8.39	8.63	156	238	157
12	36	491	9.23	8.65	8.70	107	188	268
Averages.....			8.83	8.75	8.63	201	209	225

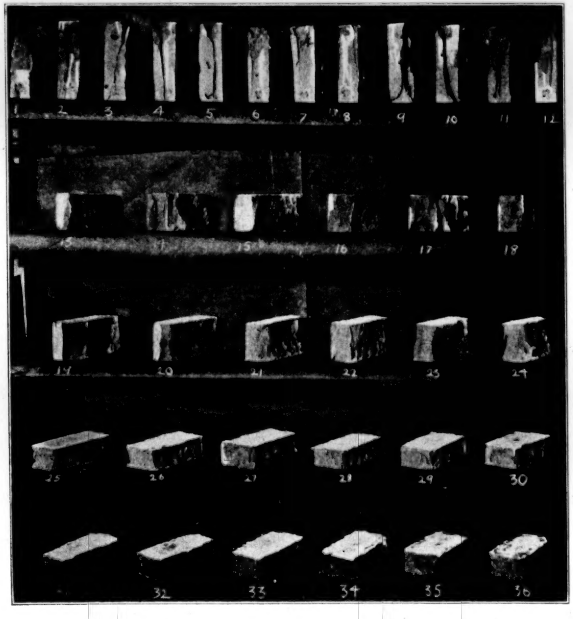


FIG. 1.—COMPRESSIVE TEST: BRICK FILLED WITH WATER.
COMPRESSION ON END, EDGE AND FLAT.

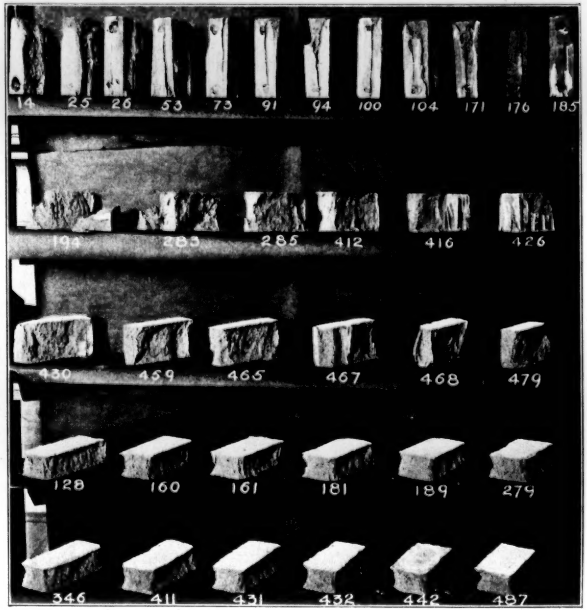


FIG. 2.—COMPRESSIVE TEST: REHEATED BRICK.
COMPRESSION ON END, EDGE AND FLAT.

rigidly to the plate, P, nor the jaws, w, but had sufficient play to allow the two jaws, w, to be separated so as to accommodate brick of varying thicknesses. With the brick in place it was fastened securely by means of the two bolts, e.

The results of the tension tests are recorded in Table No. 10. A photograph of the fractures is shown in Fig. 1, Plate VI. The results show that reheated brick will stand a higher tensile strain than those in either of the other conditions, and that brick filled with water is stronger than natural brick.

Engineers, in general, believe that the cause of failure of brickwork is due largely to tension, and, as the tensile strength of brick is so low and the chances for stress under tension so pronounced, it looks as though this was one of its weak points.

Bending or Transverse Tests of Brick.

In this test the brick were tested flat, and had no plaster of Paris or ground surfaces for bearings, but rested directly upon knife-edges in an Olsen testing machine. The distance between the lower knife-edges was 7 ins. and the top knife-edge, to which the load was applied, was $3\frac{1}{2}$ ins. from either bearing.

The results tabulated in this test are the modulus of rupture or stress in the outer fiber, p, and the deflection at the center, D. In the former case, the formula used was $p = \frac{3}{2}\frac{P\,l}{B\,h^3}$*, which is not strictly justified, but customary, in these cross-breaking tests of material having an elastic limit not easily determined. In the latter case, during the test, the deflection should have been measured, and the modulus, E, for bending, computed from the formula $D = \frac{1}{48} - \frac{P\,l^3}{E\,I}$†, which formula is true only within the elastic limit. The deflection was so small that the writer was unable to measure it, but, wishing to give some idea of the deflection in brick, he has computed the deflection from the foregoing formula, by using the breaking load and the modulus for compression. The true value of D would undoubtedly be smaller than the value obtained, as P, within the elastic limit, is somewhat smaller, and the modulus for bending considerably larger, than for compression.

* Johnson's "Materials of Construction."
† Church's "Mechanics of Engineering."

In the foregoing formulas:

P = load at center;

l = length of brick;

b = width of brick;

h = height of brick;

I = moment of inertia of the section.

The theory has been advanced that the fibers above the neutral axis of a simple beam under any loading are in compression, while those below are in tension. A comparison of the results shown in Table No. 10 with those in Tables Nos. 2 to 8, for tension and compression, respectively, will show that bricks are weaker in the former tests than in the latter. From this, it would be inferred, naturally, that, as the vertical shear at the center is zero, the brick broke in tension. Upon looking at Table No. 11 it will be seen that the stress in the outer fiber is a little more than half the total load, and is larger than that caused by tension. The following question now arises. "The excess of stress in the outer fiber, over that of tension, is taken up in what forms of distortion?" This excess of stress, in all probability, is taken up mostly in compression, while a small portion of it, undoubtedly, is absorbed by horizontal shear.

TABLE No. 11.—TRANSVERSE OR BENDING TESTS OF NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.

NUMBERS.			TOTAL LOAD AT CENTER, IN POUNDS.			STRESS IN OUTER FIBER UNDER BREAKING LOAD, IN POUNDS PER SQUARE INCH.			DEFLECTION AT THE CENTER UNDER BREAKING LOAD, IN INCHES.		
Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.
37	61	11	1 720	1 280	1 060	921	696	509	0.0026	0.0025	0.0013
38	62	89	1 060	1 440	1 800	537	736	971	0.0015	0.0026	0.0026
39	63	302	1 380	1 050	1 270	739	542	644	0.0021	0.0019	0.0017
40	64	260	920	1 370	2 000	459	733	1 097	0.0012	0.0026	0.0029
41	65	271	1 410	1 880	1 530	782	937	782	0.0023	0.0032	0.0021
42	66	370	1 470	1 500	1 510	772	809	821	0.0021	0.0029	0.0022
43	67	401	1 180	1 690	1 190	637	945	642	0.0018	0 0033	0.0017
44	68	438	2 000	1 720	1 380	1 125	1 019	730	0.0031	0.0038	0.0019
45	69	465	1 990	1 680	1 770	1 131	906	669	0.0032	0.0032	6.0015
46	70	472	1 990	1 530	1 980	1 101	775	1 077	0.0031	0.0027	0·0029
47	71	480	1 820	1 720	2 000	1 026	797	1 056	0.0029	0.0026	0.0027
48	72	488	1 990	1 030	1 960	1 126	495	991	0.0031	0.0017	0.0025
Averages.....			1 580	1 490	1 620	863	783	832	0.0024	0.0027	0.0022

FIG. 1.—COMPRESSIVE TEST:
REHEATED BRICK AND BRICK FILLED WITH WATER.
ALL LOOSE PIECES HAVE BEEN REMOVED AFTER CRUSHING.
TWO UPPER ROWS: REHEATED BRICK.
THREE LOWER ROWS; BRICK FILLED WITH WATER.

FIG. 2.—COMPRESSIVE TEST:
BRICK CUBES.
NOS. 95a TO 447b: REHEATED CUBES.
NOS. 1 TO 12: NATURAL CUBES.
NOS. 13 TO 24: CUBES FILLED WITH WATER.

Shearing Tests of Brick.

In these tests it will be noticed, upon referring to Table No. 12, that each brick was divided into halves, and that a record of the test of each half is shown. It will be seen that there is quite a difference in the strength of the two halves, showing that a single brick in itself is not perfectly homogeneous.

TABLE No 12.—Shearing Tests of Natural, Filled-with-Water, and Reheated Brick.

Numbers.			Area of section, in square inches.			Ultimate load, in pounds per square inch.								
						First half-brick.			Second half-brick.			Averages.		
Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.	Natural.	Filled with water.	Reheated.
1	25	9	8.58	8.96	8.89	583	138	349	636	179	562	610	158	455
2	26	58	8.89	8.23	8.65	371	535	497	367	705	514	369	620	505
3	27	129	8.82	8.58	8.58	380	186	262	330	385	271	355	286	266
4	28	212	8.77	8.00	8.65	381	800	509	285	625	512	333	712	510
5	29	236	8.77	8.79	8.51	274	210	544	310	319	670	292	265	607
6	30	261	8.65	8.39	8.84	453	685	543	763	353	430	608	519	486
7	31	265	8.84	8.77	8.63	279	319	513	353	506	618	316	412	565
8	32	450	8.34	8.65	8.44	528	309	461	507	116	409	518	213	435
9	33	469	8.44	8.84	8.65	585	236	370	476	411	403	530	324	386
10	34	475	7.93	8.46	8.79	908	355	165	542	693	198	725	524	182
11	35	477	8.13	8.27	8.63	492	293	554	223	235	292	358	264	423
12	36	495	8.46	8.77	8.89	212	253	484	449	380	579	330	316	526
Averages....			8.55	8.56	8.68	454	360	438	437	409	454	445	384	446

It is regretted that the moduli for shearing could not be obtained for the reason that the deflections were so small that they could not be measured.

The fractures produced by these tests are shown in Fig. 2, Plate VI. The machine used was the Riehlé, 400 000-lb. testing machine, to which was attached the apparatus shown in Fig. 6.

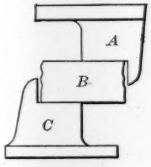


Fig. 6.

The upper shear is denoted by *A*, which was attached to the upper head-block, while the lower shear is marked *C*, and was attached to the bed-plate. The face of each shear is on the same vertical plane, which is the center of the machine. The half-brick to be tested is shown in place at *B*.

Torsional Tests of Brick.

As far as the writer knows, brick have never before been tested in this manner. Although, in actual brick construction, the stresses produced under this form of strain are very small, and practically negligible, it was thought that it would be of interest to make such a test; thus completing a set of tests of all the forms of strain to which a brick can be subjected under ordinary engineering operations.

In conducting this test, the Thurston Autographic Testing Machine, sometimes spoken of as Thurston's Torsion Testing Machine, was used.*

The machine consists, essentially, of a pendulum, a drum, a curved guide by which a pencil arm is moved and traces upon the drum a permanent record, as shown in Figs. 9, 10 and 11. The test piece is held in place between the pendulum and the drum by two jaws.

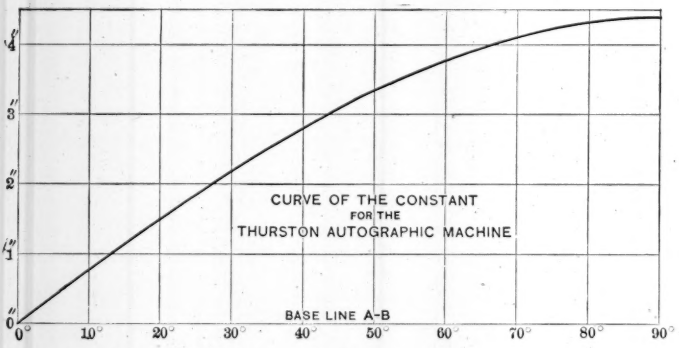


Fig. 7.

In carrying out the torsional tests the writer first standardized or found the constant of the machine, as follows: A piece of paper was first placed in the drum while it was unclamped on the shaft, with the upper end of the pencil rod in the hollow of the curved guide and the pencil on the drum, while it was turned through 180°, more or less, thus ruling the base line *A B*, as shown in Fig. 7, from which the ordinates are measured. As the pendulum was raised to a horizontal position, the pencil traced, on the paper in the drum, which was now clamped, the curve of the constant, as shown in Fig. 7. The pendulum

* A description and illustrations of the working parts of this machine may be found in Thurston's "Materials of Engineering," pp. 378 *et seq.* Reference may also be made to Carpenter's "Experimental Engineering," p. 96.

FIG. 1.—TENSION AND BENDING OF BRICK.
THREE UPPER ROWS IN TENSION.
THREE LOWER ROWS IN BENDING.

FIG. 2.—SHEAR AND TORSION OF BRICK.
THREE UPPER ROWS IN SHEAR.
THREE LOWER ROWS IN TORSION.

in the horizontal position was found to weigh 121 lbs. at the center of gravity of the weight, which was 4.16 ft. from the center of the head. The circumference of the drum was found to be 36 ins., which indicated that each inch on the base line was equivalent to an angular distance of 10° for both pencil and pendulum.

Sufficient data having been obtained, the constant of the machine was derived, as follows:

$P\,a = 121$ lbs. $\times$ 4.16 ft.

$= 503.36$ foot-pounds, the moment for the 90° position of the pendulum.

The length of the ordinate for the 90° position is 4.4 ins., and, as the constant of the machine is 503.36 foot-pounds, 4.4 ins. = 114.4 foot-pounds per inch of ordinate. The presence of some journal friction would necessitate this being accounted for in determining the final results. The pull at the center of the weight and 4.16 ft. from the center of the drum, to overcome the journal friction and hence pull the pendulum from its vertical position, was equivalent to $1\frac{7}{8}$ lbs. The constant for friction, or the frictional moment, is $1\frac{7}{8}$ lbs. $\times$ 4.16 ft. = 7.80 foot-pounds.

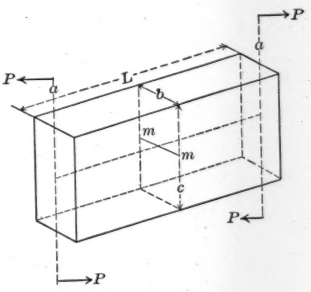


FIG. 8.

The test piece being rectangular in section, the theoretical results obtained by Grashof were taken. See Fig. 8.

Let L = length of brick actually subjected to torsion;

P_s = maximum shearing stress in the outer fiber (which will occur at the middle of the long side, at m) $= \frac{9}{2} \times \frac{P\,a}{b_2}\,c$;

α = angle of torsion, which (in π measure), for a length, L,

$$= \frac{9}{2} \times \frac{P\,a\,L}{E} \times b^2 + \frac{c^2}{b^3}\,c^3.$$

Of course, these formulas only apply if the elastic limit is not passed. To use them beyond this limit gives only approximate results, but, nevertheless, this is the best that can be done, under the circumstances.

TABLE No. 13.—TORSIONAL TESTS OF NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.

Shearing Stress in the Outer Fiber.

NUMBERS.			NATURAL BRICK.		BRICK FILLED WITH WATER.		REHEATED BRICK.	
Natural.	Filled with water.	Reheated.	Constant.	Stress under breaking load, in pounds per square inch.	Constant.	Stress under breaking load, in pounds per square inch.	Constant.	Stress under breaking load, in pounds per square inch.
37	61	13	0.229	414	0.219	359	0.223	559
38	62	143	0.225	526	0.229	503	0.233	470
39	63	203	0.219	532	0.229	462	0.223	324
40	64	206	0.225	402	0.221	258	0.242	659
41	65	211	0.251	750	0.221	261	0.163	325
42	66	219	0.247	817	0.225	368	0.217	422
43	67	388	0.253	626	0.231	370	0.169	410
44	68	402	0.249	724	0.275	916	0.229	608
45	69	455	0.233	552	0.273	759	0.213	863
46	70	456	0.251	658	0.231	598	0.213	420
47	71	489	0.244	517	0.264	734	0.213	410
48	72	493	0.238	790	0.239	632	0.219	381
Averages.....			0.239	609	0.238	518	0.213	487

TABLE No. 14.—TORSIONAL TESTS OF NATURAL, FILLED-WITH-WATER, AND REHEATED BRICK.

Modulus of Elasticity for Shearing.

NUMBER.			NATURAL BRICK.		BRICK FILLED WITH WATER.		REHEATED BRICK.	
Natural.	Filled with water.	Reheated.	Constant.	Modulus, in pounds per square inch.	Constant.	Modulus, in pounds per square inch.	Constant.	Modulus, in pounds per square inch.
37	61	13	6.429	49 900	6.021	32 900	6.127	115 200
38	62	143	6.251	39 800	6.271	137 900	6.524	32 200
39	63	203	5.998	36 400	6.415	103 400	6.127	28 100
40	64	206	6.227	14 200	6.125	45 200	6.497	55 900
41	65	211	7.175	13 800	6.097	32 000	4.229	36 100
42	66	219	6.999	29 200	6.227	27 200	5.924	55 400
43	67	381	7.210	21 800	6.394	76 900	4.391	53 300
44	68	402	7.015	26 000	7.961	45 500	6.360	45 700
45	69	455	6.647	21 700	7.889	59 900	5.786	104 100
46	70	456	7.202	19 400	6.493	106 100	5.847	115 200
47	71	486	7.033	25 900	7.620	46 300	5.803	89 300
48	72	493	6.698	19 300	6.725	42 700	6.044	90 200
Averages.....			6.741	26 500	6.687	63 000	5.810	68 400

Tables Nos. 13 and 14 show that there has been found by torsional tests, shearing stresses in the outer fiber, and the modulus of elasticity for shearing, in pounds per square inch, under the breaking load, the values in these tables being for the ordinates shown in dotted lines in Figs. 9, 10 and 11.

In arranging these two tables, both constants and stresses were given for each set of brick, thus enabling the reader to find the stresses for other ordinates of the curves of torsion, by means of the constant and measurement of ordinates and abscissas. The constant in the table of shearing stresses is equal to $\frac{9}{2} \times \frac{1}{b^2 c}$, which is a portion of the formula, $p\sigma = \frac{9}{2} \times \frac{P a}{b^2 c}$; while the constant in the table of moduli is equal to $\frac{9}{2} \times \frac{L}{\alpha_1} \times \frac{b^2 c^2}{b^3 c^3}$ (α_1 being the value of 10°, expressed in π measure), which is a portion of the formula,

$$\alpha = \frac{9}{2} \times \frac{P a L}{E\sigma} \times \frac{b^2 c^2}{b^3 c^3} = \alpha_1$$

times the length, in inches, of the abscissa in question. The shearing stress for any ordinate, therefore, is the constant of the machine, expressed in inch-pounds, multiplied by the length of the ordinate, in inches, plus the frictional constant, in inch-pounds, which sum is $P a$, in the formula, and is multiplied by the constant in Table No. 13. The modulus for any ordinate is $P a$, divided by the corresponding abscissa, multiplied by the constant in Table No. 14.

The photograph, Fig. 2, Plate V, shows the specimens which were submitted to torsional tests—natural brick in the upper row, reheated brick in the middle row, and brick filled with water in the bottom row. In all these three tests it will be seen that the corners gave way first, followed in most cases by a series of thin parallel curved planes. In no case did the failure take place in the central portion of the brick, nor, with such a brittle material, is it ever likely to take place there, unless the area of this portion is reduced considerably from what it is at the points where the test piece is held.

The distance the grips extend into the brick would have some bearing upon the number of parallel planes broken off and the distance of these planes from the corner. The farther the grip extends into the test piece, the more remote from the corner will the planes break off,

and the greater will be their number, while the distance between the parallel planes remains the same. The tendency in the brick is to break off at both of its corners at one end and turn around, with the center of each end as an axis, as in Brick No. 46, in Fig. 9. Sometimes the corners on ends diagonally opposite each other, or on the same edge, will fail first, leaving the brick between the grips, in a skew or square, respectively, as shown in Bricks Nos. 219 and 66, Figs. 10 and 11. In the foregoing tests the grip or jaw extended into the brick, which was held in place by wedges.

By examining the curves of torsion in Figs. 9, 10 and 11, it will be seen that there is more or less irregularity in the curves; indicating that the brick failed all at once or by degrees. The most regular curves are where they failed at once; the irregular curves are where they failed by degrees. The more the irregularity of the curve, the greater number of stages during failure, and, hence, the greater number of thin parallel planes.

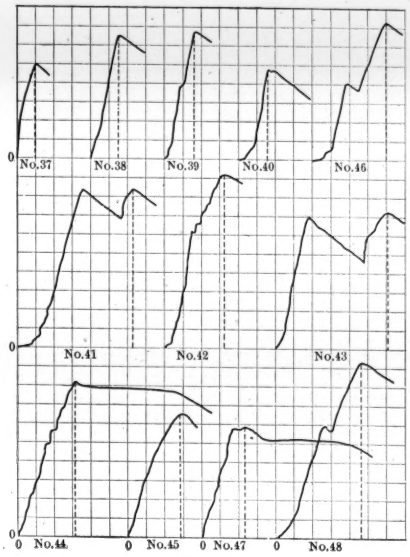
By studying Tables Nos. 13 and 14, and the curves of torsion, it will be seen that the natural brick stood the highest in shear, and the reheated brick in moduli. The highest special brick in shear is No. 68, tested filled with water; while the highest in moduli is No. 62, which was also tested filled with water. It will be noticed that those bricks having curves with the longest ordinates are the highest bricks in shear, while those which have the shortest abscissas are the largest in moduli.

Conclusion.

In comparing the tables for the strength of the brick under different strains, one finds the following order of increase in strength, namely, tension, shear, torsion, bending, compression.

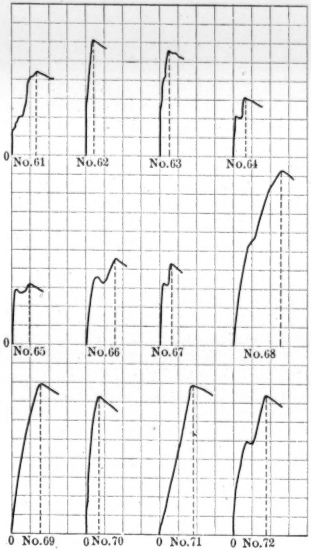
This order would naturally lead one to believe that the least important test was that of compression, and the most important that of tension, the others occupying places in the order given. But this does not follow, necessarily, for strains set up in certain directions are more marked than in others. For example, torsion would stand third in importance if rated in this way, but the writer believes this to be the least important test, because, in brickwork, torsional strains are less likely to be set up than any other form of strain.

The most important tests are tension and compression, as these strains are more likely to occur than any others, and, of these, tension



CURVES OF TORSION
FOR NATURAL BRICK

FIG. 9.

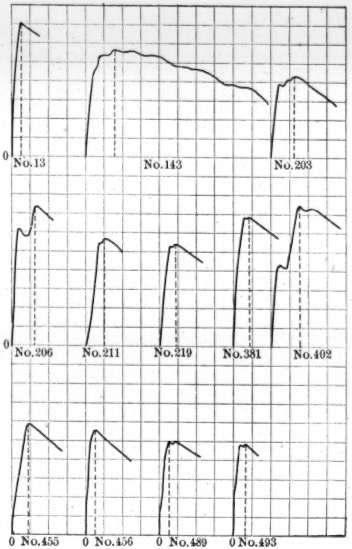


CURVES OF TORSION
FOR BRICK FILLED WITH WATER

Horizontal Scale
Degrees
0 5 10

Vertical Scale
1 inch
0 ½ 1

FIG. 10.



CURVES OF TORSION
FOR REHEATED BRICK

FIG. 11.

is the more important, as it is the weaker stress, and, when brickwork fails, bulging is brought out very prominently, and this produces tensional strains, and, to a less marked degree, bending and shear. Of the two remaining forms, bending is more important than shear, as strain along this line is more likely to occur than in shear, although shear shows the weaker stress.

As to the best kind of brick to use, the tables and the curves of distortion show results decidedly in favor of reheated brick; but, taking into account the expense of preparing reheated brick and the loss of 12% of them, it would be cheaper to use enough extra natural brick to make up the excess of strength found in reheated brick, unless the reheating has been done in a conflagration. Natural brick and brick filled with water show about the same results, so that, in general, the water does not impair the strength of the brick.

In the compression test, the brick on flat are the strongest, while the brick cubes are the weakest, and the brick on end are weaker than those on edge. In this test the pieces are 2, 4 and 8 ins. in height, from which the strength for a square inch for the respective heights can be computed. The question comes up: What would be the strength of each of these specimens if it were only 1 in. high? The writer has assumed that the specimens would increase in strength per square inch as many times as the specimen is inches in height, and has, as a result, the figures given in the columns headed, "Equivalent ultimate load per unit of height, in pounds per square inch." The results show that this assumption is wrong, and that some other ratio of the height exists. What this is can be found more satisfactorily by tests than by assumptions.

By looking at the shearing and torsional tests, Tables Nos. 12 and 13, respectively, it is seen that those in Table No. 12 are for direct shear, and those in Table No. 13 are for shear in the outer fiber, caused by torsional strain. In the case of reheated brick, the stresses are about the same in both tables, those in No. 13 being a little larger, while the greatest difference is in the natural brick, those in Table No. 13 being about one and one-half times those found in Table No. 12. In these two tables the important test is that of direct shear.

DISCUSSION.

E. J. McCaustland, Assoc. M. Am. Soc. C. E. (by letter).—In conducting these tests, which show a great amount of painstaking labor on the part of Mr. Turrill, an excellent opportunity was offered to determine the rate, as well as the total amount, of absorption. With bricks heated for the time, and to the degree indicated in these experiments, and with the time allowed for the complete test, valuable information might have been obtained by weighing the bricks at frequent intervals during the period of immersion. Mr. McCaustland.

There is little doubt that the rate of absorption is much greater during the first few hours of immersion than at any later time, although there will be increasing absorption for a long period.

In ordinary commercial work it is not usually convenient to extend the absorption tests over more than 48 hours, and sometimes not more than 24 hours. The latter time is rather short; but the writer believes that 48 hours gives sufficient time to indicate clearly the character of the brick and give results definite enough for all purposes of comparison. It is probable, in any case of building brick and most cases of paving brick, that at least 75% of the total water of absorption would be taken up during the first 48 hours of immersion, and it is believed that this initial absorption affords fully as satisfactory an index of the character of the brick as would be obtained from a more extended test. Certainly, it is absorption in the first stages that is important to consider in ordinary brickwork where it is exposed to moisture. Brickwork under water is usually constructed of a material which will absorb only a very small percentage of its own weight.

The writer believes that tension tests of brick should receive more attention than has been given to them heretofore. In crushing tests of a large number of brick piers or columns, he has found that failure occurred invariably by splitting longitudinally, the individual bricks failing by tension, with little or no indication of crushing.

The author's conclusions, based upon the results of his tensile tests, concerning the comparative strength of natural, filled-with-water, and reheated bricks seem to be scarcely justified. It is true that on the twelve tests the average strength of the reheated bricks is highest; but it is to be noted that two of these specimens (Nos. 96 and 231) give abnormally high results. With these omitted, or scaled down to the next highest figure (No. 491), the average will be practically the same as for the natural brick. Comparing the "filled-with-water" and the "natural," it is to be noted that six of the latter give a higher value than the corresponding "filled-with-water," and on the whole set of twelve bricks the averages for the three conditions differ only 8 lbs.

Mr. McCaustland. It would seem to the writer more nearly correct to conclude that these tests do not establish any difference whatever in the tensile strength of these three classes of brick. The stresses due to bending, although not to be classed as tensile stresses, have some significance in this connection, and their averages indicate an excess of strength in favor of the natural brick. However, notwithstanding the extent of these experiments and the care with which they were conducted, they are not sufficiently numerous to warrant general conclusions.

Mr. Turrill. SHERMAN M. TURRILL, Assoc. Am. Soc. C. E. (by letter).—Admitting as true all that Mr. McCaustland has said, the writer would like to add some explanation, in order that there may be no misunderstanding. When the absorption tests were planned, the writer was aware that in commercial work forty-eight hours was sufficient time for a brick to remain under water, and that it would absorb more water during the first stages of its immersion, but he was interested most in ascertaining the greatest amount of absorption. If the greatest amount of absorption is within the proper percentage, the brick is safe at all stages.

In these tests the writer was advised to confine himself to brick of one make, and to test that thoroughly, thus laying a foundation for the comparison of tests of other makes, and this he did.

In making the tests their full commercial value was not always considered, and they were carried out more on a scientific than a commercial basis.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INSTITUTED 1852.

TRANSACTIONS.

Paper No. 958.

TIMBER TESTS.

An Informal Discussion at the Annual Convention, June 10th, 1903.

By Messrs. W. K. Hatt, Hermann von Schrenk, Gaetano Lanza, A. L. Johnson, S. Bent Russell and W. K. Hatt.

Mr. Hatt.

W. K. Hatt, Assoc. M. Am. Soc. C. E.—The speaker, who is on the staff of the Bureau of Forestry in the work of organizing the projected investigation of the strength of timber, has been asked to represent the Bureau of Forestry in this discussion. A general statement of the character of the investigation has been prepared by Gifford Pinchot, Assoc. Am. Soc. C. E., Forester, which is here reproduced as published in the *Proceedings** for April, 1903.

"The Bureau of Forestry, of the United States Department of Agriculture, intends to resume the work of testing timber formerly carried on by the Division of Forestry during the years 1891–1896. A large portion of the groundwork covered by the proposed investigation is of interest to engineers, and it is the desire of the Bureau that the results obtained shall be of the greatest value to them. To this end it is hoped that those engineers interested in the application of timber to construction, will, in this discussion, offer suggestions and make criticisms which will aid the Bureau in planning this series of tests.

"It is the intention of the Bureau to take up these tests in a thorough manner, and devote whatever time may be necessary for the

* *Proceedings*, Am. Soc. C. E., Vol. XXIX, April, 1903, p. 133.

Mr. Hatt. solution of the problems involved. It is expected that the plan of operation will be sufficiently complete, and the methods of tests so well chosen, that the data obtained may be accepted by all experts.

"The following account of the proposed tests is of a very general nature. Those who are particularly interested can obtain a description of the proposed tests in greater detail on application to the Bureau of Forestry, Washington, D. C.

"The Bureau aims at results of practical value, such as the values expressing the strength and stiffness of the principal merchantable species of timber of this country. Not only will this be done for timber now in use, but efforts will be directed to ascertaining the value for structural purposes of timbers at present not largely used, as, for example, the Western Hemlock and the Southern Gums. A thorough series of tests will be made of the timbers of the Pacific Slope, particularly the Red Fir, otherwise called Oregon Pine or Douglas Spruce, which is now finding its way in increasing quantities to eastern markets. If it should appear that the work done on the Southern Pines by the Division of Forestry during the years 1891–1896 has been sufficiently complete, the proposed tests will not cover these timbers.

"Experimentation will be directed also in advance to the investigation of the effects of different factors of the testing processes on the results, such as speed of application of a load, method of moisture determination, the effect of moisture, and of volatile oils.

"The investigation of the effect of technological operations, such as methods of kiln drying, methods of preserving timber, etc., will be undertaken.

"In determining the values of the strength and stiffness of various merchantable timbers, tests will be made, both on timber collected from the market and on timber collected in the forest. Full-sized sticks, as used in construction, will in all cases be tested under their appropriate loadings.

"Work is at present in progress at a station in California, on the different merchantable grades of these full-sized timbers, such as bridge stringers and car sills collected on the market. The locality of origin of the timbers is determined, and a full description of each stick, as well as a series of photographs of all six sides, is made a matter of record. Full information of the physical and mechanical properties of the sticks under test will be obtained.

"The investigation of the effect of various conditions of forest growth on the physical and mechanical properties of timber will be undertaken whenever the Bureau is thoroughly prepared for this work.

"It is hoped that the discussion will take up the need of making timber tests from the standpoint of the engineer. Suggestions are invited as to those particular matters which at present most need investigation. It is also desired that discussion shall take place concerning the methods of performing the tests, particularly as to the best method of determining the moisture in timber; and the best method of determining the shearing strength of timber.

"It is hoped that a full criticism of the former tests, both as to methods and content, will be given.

"It is recognized that timber is subject to a great number of variables. From the botanical standpoint, the ideal method of procedure would be to determine the properties of the wood fiber of each species representing the normal growth of straight-grained timber. With these standard values as a basis, inquiries could be directed to

ascertain how much the values are affected by such variables as moisture, oil, speed of test, imperfection of structure due to direction of grain, or knots, rate of growth, diseases of the tree, technological processes, such as kiln drying or preserving, etc.; forest conditions, region and growth. It would seem that the interests of all, whether engineers, manufacturers, or botanists, would be consulted by such a method of procedure. The particular series of tests on full-sized sticks of market timber would form part of this general scheme." Mr. Hatt.

The Bureau has sent out a preliminary circular, to many engineers and others interested in the utilization of timber, in order to determine the need of future tests and to formulate a plan of procedure. After the content of the investigation and general plan is determined in its general features, by such correspondence and preliminary discussion, it is the intention to perfect the arrangement and details of this plan, involving methods of measurement and experiment, and submit it to experts for a record of agreement or disagreement with the proposed methods of test. The plan and methods, as far as practicable, will be of such a character that the work may be undertaken in part, and the partial results obtained will be of use throughout the entire investigation.

The results of the preliminary canvass of those interested in the utilization of timber will be published in due time. The replies to the circular make evident the fact that, far from the belief that the problems connected with timber are not serious because of a partial and growing replacement of timber by other material, there is a lively appreciation of the importance of a continuous organized study of methods which will economize the use of timber, lengthen its life, and develop the supply by proper forestation. The matter of determining the strength values of timber is only a minor part of the general need. In view of the variability of the product, the details of testing operations are considered by many to be of minor importance.

Such useful lines of work as the following have been suggested :

The need of proper determination of the strength of the Pacific Coast woods, Fir, Spruce, Hemlock, and the Eastern Spruce and Hemlock, which will form the future supply of structural timber, is suggested from many quarters. The determination of the mechanical properties of the various hardwoods which probably will form substitutes for Hickory and White Oak in carriage construction is a matter of importance.

The majority of engineers would be more satisfied with the results of tests of the timber actually supplied to the market by saw-mills (timber of prime quality, average quality, merchantable, square-edged, etc., as modified by the forms and proportions of sticks and by various degrees of seasoning), than by tests conducted on timber collected from the forest and sawed up at the order of the engineer of tests, in

Mr. Hatt. order to obtain a solution of the various problems which confront the forester and the botanist. One prominent user of timber insists that the quotation of the strength of timbers should be made with reference to the sizes and lengths of sticks, as in his experience the unit values to be used in design should be much smaller as the sticks become larger. The degree to which the values obtained from tests on new material are to be reduced by the inevitable decay which results from their use, is another matter which has been suggested.

The need also is pointed out of uniform standard rules of inspection and grading, involving the determination of the value of the reducing factors due to knots, crooked grain, and sap. A useful work is suggested in the presentation of photographs, etc., of the appearance of sawed lumber of different species.

Shippers demand a determination of the average weight of different species of timber, at different degrees of seasoning, as a basis for the computation of freight charges.

All unite in the expression of the usefulness of an investigation of the proper methods of preserving timber and the most advantageous species to be used in cases where the timber is to be preserved. Questions of the relative life of treated lumber to be expected under different exposures, and the question of the strength of such treated lumber, are also matters of importance to many. The proper methods of seasoning, or hastening the seasoning process, of both hard and soft woods, is a matter which might be investigated with great profit.

From the standpoint of the forester, the effect of the more important forest conditions upon the strength of timber is of scientific and practical interest. Such problems as the relative strength of swamp and highland hardwoods, first- and second-growth timbers, etc., suggest themselves; and a demonstration of the appropriate uses of these inferior timbers, which at present are left uncut in the forest, and which improperly influence the future character of the forest, will be of use to the forester.

It is evident that the subject of timber investigation is a formidable problem to attack. The ideal procedure would involve a systematic programme in advance, including all the problems, so that one part of the field may be covered at one time and the results afterward may fit the general scheme. This plan, of course, would involve testing timber as found in the forest, in which all the conditions are known. And yet, while this is true, the speaker agrees with those who believe that the most direct method of getting unit strength values for the engineer is to test timbers in actual sizes as found on the market. There is some waste of time and duplication of work in this manner of procedure, from the standpoint of the complete investigation of all problems, but the usefulness of the results to the parties for whom the tests are designed, will practically be much

greater than by any other method. The progress of knowledge derived from experiment will thus take place in the same manner as in the case of other materials. In no case has the entire subject been cleared up in a single investigation, however ideal that procedure would be. In all cases care should be taken to determine the species of timber under test, and to ascertain the region of growth. There is no difficulty in determining the species and region of growth from the records of the lumber dealer, in the case of large timbers. The weight, rate of growth (as shown by annual rings), and the defects, can be made a matter of record. A complete study of species, from the botanist's standpoint, would involve the detailed examination of the entire tree as cut in the forest. Mr. Hatt.

The following discussion aims at a summing up of the recent additions to our knowledge of the mechanical properties of timber, and the consideration of the directions in which further experimentation should be made, both to determine mechanical properties as affected by different conditions and to improve methods of preserving timber and of technological processes. The discussion does not concern itself with details of methods of test and measurement. Propositions in regard to these details will be presented to the American Society for Testing Materials, at the meeting at Delaware Water Gap, on July 1st to 4th, 1903. Nor are dendro-chemical problems mentioned.

Recent Additions to Knowledge.

The strength of the different species of timber of the United States, used in construction, as known up to the year 1895, is very well summarized in the Report of a Committee of the American International Association of Railway Superintendents of Bridges and Buildings, presented before the Convention at New Orleans, on October 16th, 1895. This report presents the results of an exhaustive compilation by Walter G. Berg, M. Am. Soc. C. E., Chief Engineer of the Lehigh Valley Railroad. The values obtained by different experimenters are listed in a number of tables. This report is reproduced in Bulletin No. 12 of the Division of Forestry, United States Department of Agriculture. A more complete table of mechanical properties of a wider range of species is contained in the Report of the Tenth United States Census. A summary of the results accomplished in the former United States Government Timber Tests will be found in "The Materials of Construction," by the late J. B. Johnson, M. Am. Soc. C. E., both as to moduli of strength and effect of various factors. This summary is more complete than any given in the interrupted official publications of the Division of Forestry.

Since 1895 the experiments worthy of note are those on White Pine, Red Pine, Hemlock and Spruce by Professor H. T. Bovey, of

Mr. Hatt. McGill University,* showing that in many cases no increase of strength can be counted upon due to the reduction of the moisture from a green to a kiln-dried state, in the case of large beams. This contradiction of the indications from tests on small sticks is due to the checking of the beam during drying, and subsequent failure under longitudinal shear.

Additional information concerning the mechanical properties of species is contained in Circular No. 19 of the Division of Forestry, United States Department of Agriculture, dealing with the strength of Bald Cypress.

William Hood, M. Am. Soc. C. E., Chief Engineer of the Southern Pacific Railway, has made an extensive series of tests of full-sized sticks of the Red Fir, with a view to obtaining unit values for timbers of different sizes, and the effect of creosoting on the strength. He finds that both the modulus of elasticity and the fiber stresses must be diminished with increasing depth of beam. Thus, for a depth of 2 ins., the modulus of elasticity is 1 793 000 lbs. per square inch, the elastic limit is 6 950 lbs. per square inch, and the modulus of rupture is 11 200 lbs. per square inch; whereas, for a depth of beam of 16 ins., these values, as figured from the load at first indication of failure, are 1 074 000, 3 769 and 5 590 lbs. per square inch, respectively. In Mr. Hood's tests, however, the reduced values for larger sticks are due to the effect of the relatively short span in bringing about failure under longitudinal shear. There is no decided indication of actual diminution of fiber strength at the center. However, the practical use of beams in the ratios of span to depth equal to those used under test conditions, make the results reached by Mr. Hood of significance in determining the ratio of depth to span at which the beams must be designed for longitudinal shear. He finds in the case of 8 x 10-in. creosoted Red Fir beams, that the modulus of elasticity, the elastic limit and the modulus of rupture are 0.973, 0.826, and 0.606, respectively, on the basis of unity as the value for untreated timber. The reduced strength is mainly to be accounted for by the presence of checks arising from the creosoting process, and from the diminution of the shearing strength of the wood. These influences determine a diminution of the strength of the large beams compared with similar large beams.

L. E. Hunt, Assoc. M. Am. Soc. C. E., of the University of California, has demonstrated the diminution of shearing strength of creosoted Red Fir timber. He finds that the other quantities, elastic limit, modulus of elasticity, etc., are but little affected. These same statements apply to Burnettized timber.

A valuable series of tests on full-sized timbers and structural details has been in progress at the Laboratory of the Massachusetts Institute

* *Transactions*, Canadian Society of Civil Engineers, Vol. XIV, 1897.

of Technology, under the direction of Professor G. Lanza, with a view to obtaining unit stresses appropriate to market timbers. Mr. Hatt.

A series of tests of Redwood and Red Fir, from sticks of 4 x 4 ins. in cross-section, has been made under the direction of Professor Soulé, of the University of California.

A comparison of the mechanical properties of the Red Fir and Southern Yellow Pine, as based on sticks 4 x 4 ins. in cross-section, has been made at the Bremerton Naval Station by Naval Constructor Frank W. Hibbs, showing that these two woods are about equal in value, as far as the mechanical properties are concerned.

In general, it may be said that our present knowledge of the average mechanical properties of the different species of structural timber is fairly complete, as far as such values may be based on small sticks. The reductions which must be made in case of large timbers of the different merchantable grades, and the manner in which these values are affected by forest conditions, are largely unknown. The effect of moisture on the mechanical properties of wood fiber has been well determined, but the proper reducing factors for the effect of moisture, in case of large sticks, are not known. Nor are the moisture contents of market timber well known. It is fairly well established that, in woods of uniform structure, the strength increases with specific weight. The relations between the mechanical properties of any one species are only partially established. The variations of the physical and mechanical properties with position in trunk are well established in the case of the Southern Pines.

The outline of any future work should not duplicate work already performed in a reasonably satisfactory manner. Tests should be confined to species that give promise of being on the market for some time. Before presenting an outline of the series upon which investigations may profitably be made, some discussion will follow on the various factors which enter into the problem.

Technological Processes.

Kiln Drying.—The effect of hot-air drying on the strength was investigated in the former United States timber tests. It was found that, for any temperature commonly used in drying lumber, no detrimental effect on the strength would be produced, aside from the checking action which might result from too rapid drying of the exterior portion of the stick. The effect of very high temperatures and pressures used in drying, as in the vulcanizing process, is, according to the former Government timber tests, to reduce the strength slightly.

The subject has not been well covered, and an investigation of the temperature and rate of drying, to determine the best conditions for the prevention of checking, together with a general study of the

Mr. Hatt. methods of seasoning, is planned. This, however, is outside the realm of mechanical tests, as is a projected study of the behavior of timber in the operations of steaming and bending.

Bleeding for Turpentine.—The effect of bleeding Long-Leaf Pine for turpentine is not detrimental to the strength of the timber, according to the results of the previous Government mechanical and chemical tests. These tests are believed to be conclusive, and will not again be taken up, except to determine the effect of this operation on the durability of the timber.

Fire-Proofing.—Other matters which are to be taken up in the proposed investigation include a study of the effects of fire retardants, and their efficiency for various species of timber, as well as the effect of these on the mechanical properties, and on nails and fastenings.

Preservatives.—A study of the durability of the different woods in their natural state under ordinary service conditions will be useful in giving information as to what species of wood are suitable for given conditions. A large field for investigation is the efficiency of preservatives in the treatment of the so-called inferior woods, such as second-growth Loblolly Pine, Hemlock, Red Oak, Beech, Eucalyptus and the Gums. This most valuable study will be outlined after consultation with those at present studying the subject. It is desirable that laboratory tests be devised to avoid the excessive cost of field tests.

Variations in Quality, and Imperfections.

It is the common belief that tests on small sticks do not yield unit values applicable to large sticks. It is supposed that the small pieces selected for test are unavoidably of a better average character than large sticks; very often these small pieces are clear and straight-grained. Experimental evidence seems to show that the results of tests by those who test large sticks are lower than the results obtained by those who have experimented on smaller sections.

It seems reasonable to suppose that the modulus of elasticity and the elastic strength of identical material should not be affected by the size of the specimen, provided that the ratio of depth of beam to span be great enough to eliminate largely the effect of shear. It is known, however, that two pieces of wood are far from identical. In spite of this heterogeneous character of timber, however, small and large sticks may yield equal moduli, for the reason that the effect of a knot of given size is more serious in a small stick than in a large stick. And this consideration may explain the fact that, although timber in small sizes is usually more free from knots and other defects than timber of large sizes, the comparison of results of tests of a large stick with a number of tests on small sticks cut from this same large stick shows that there is no significant difference in the average unit values

from the large and small sticks. This is what the former Government timber tests have shown. Mr. Hatt.

What the designer wishes to know, however, is the most probable strength of the different sizes of timber and different grades of timber that he is compelled to purchase on the market, as, for instance, the different grades of car sills, stringers, and carriage material. It seems to the speaker that this knowledge can only be reached through a direct test of the actual market timbers, the qualities of which are determined by the saw-mill. A given log is cut in such a way as to furnish the desired products to the best advantage. It is less likely that logs sawed up under the direction of the engineer of tests will yield results comparable with these commercial products, which represent the combined judgment of a great number of engineers and lumbermen. The relation of the strength of large sticks to the small sticks cut from them does not necessarily agree, for instance, with the relation of the quality of a railway stringer and car sill, both of first-grade merchantable timber, cut from a given log.

Useful information will be presented by the proposed method of photographing the sticks tested, so that the effect of different kinds of knots, and the position of these, upon the strength of the different kinds of test pieces may be determined by the individual who examines the reports. In the publication of results of tests, analyses should be presented to determine in some quantitative measure the reduction of the moduli due to the effects of knots, shakes, and crooked grain. Information will also be available concerning the liability of various species to these defects. The former Government publications relating to timber tests have been criticized because no record was made of the quality of the sticks. This criticism will be met in the future by the publication of photographs of the sticks, showing the grain and defects and a quotation of the market grade of the stick. Tests are now under operation, and will be continued, on sticks of various sizes and grades, as purchased on the market.

Forest Conditions.

The influence on the quality of timber of the various forest conditions, such as climate, winds, rain, seasonal variations, soil, elevation, rate of growth, density of stand, time of cutting, position in tree, etc., is of interest, and the determination of the effect of the more important of these should be included in any extended study of timber, to the end that in forest management or in reforestation the best results may be obtained, or that present prejudices may be removed. This formidable investigation has scarcely been entered upon in a formal way, although a large body of information is in existence as the result of experience and observation.

Mr. Hatt. No useful results have as yet attended the attempts to solve many of these problems, by European investigations, or by the former Government Timber Tests. Many of the factors seem to be of such minor importance that their influence is necessarily obscured by the inevitable differences in test pieces obtained under apparently identical conditions. It also seems impracticable to obtain any modulus which will represent the mechanical properties of a tree grown under given conditions. Knowledge of the effect of forest conditions, in the speaker's opinion, must be largely inferential, and not precise. A practical method of obtaining these relations would involve, first, a determination of the mechanical qualities of timber of various grain and weight; the forester or botanist can then, by an inspection of the wood under examination, in the forest, decide its value by reference to the tables prepared for him as a result of previous mechanical tests. This, however, is only the speaker's personal view.

One useful outcome of this investigation might be the determination of the most favorable condition for the production of any given species, as to the various conditions, such as soil, density of growth, elevation, climate and seasonal changes. It is possible, however, that this matter is not of immediate practical importance, because in the United States, for many years to come, trees must grow under conditions little changed from those of the present. Useful conclusions can be reached in pointing out the relative value of timber grown in different regions of the country, as, for instance, the relative strength of Northern and Southern Hickory, of first- and second-growth hardwoods, of Coast and Mountain Red Fir, of first- and second-growth Loblolly. The timber, as found in the market, with its important features of weight and rate of growth known, will serve this purpose in general.

The tests on inferior species are of great importance, because they will undoubtedly tend toward a greater utilization of the inferior woods. If these inferior species can be cut and marketed at a profit, it would be a great help to the future sylvacultural condition of the forest itself. As it is now, usually only the most valuable species are cut, leaving the inferior ones. As a result of this, the future forest is composed largely of the poorer class of trees. If these inferior species can be largely cut, and a small percentage of the valuable species left standing, the chances are much better that the new growth will contain a large percentage of the valuable woods. The Bureau has not yet planned a formal investigation of the effect of forest conditions, and does not contemplate taking this up until more pressing matters are disposed of.

The matter of supplementing the former Government timber tests by tests on inferior woods, in order to determine strength moduli, is also important, for the reason that some of these inferior woods have

replaced the better grades, and will continue to do so at an increasing rate on account of the increasing price of the latter. For instance, architects are now using, for indoor service, 8 x 16-in., square-edged timbers, of second-growth, Loblolly Pine, of almost entirely sapwood, of such rapid growth that four annual rings occur in 3 ins. at a point more than half way between the heart and the outside of the stick. It is evident that the results of tests on first-growth merchantable Pine of the best grade will not apply to such material as this. Mr. Hatt.

Joints and Frames.—Tests to determine the strength of structural details, such as joints and frames, may be left to private enterprise.

Variations in Testing Process.

Rate of Loading.—At present it is known that the rate of application of the load exercises a very important influence upon the shape of the load-deflection diagram, particularly at the loads just preceding rupture. It is known, also, that the deflections under the ordinary quickly-applied load in a test are only one-half of those resulting from the continued application of the same load. The quantitative relations are not well determined, and a further study is imperative, and has been planned.

Moisture.—The method of determining the moisture contents of the sticks under test, and the use to be made of such moisture determinations in reducing the moduli to a common basis of moisture, are matters that excite a great deal of discussion on the part of operators of tests. A series of tests should be made, to determine the proper reducing factors in the case of large timbers, as between the green and dried states. While the results of tests on small-sized specimens show that the strength of dry timber is nearly twice that of green timber, it is probable that in many cases no such increase is to be expected, on account of the diminution of the shearing strength due to the checking action arising during the drying-out process. An investigation will be made to determine the relation between the different methods of determining moisture in wood, and the disturbance of results due to the presence of volatile oils in the material under examination. At present it appears that the latter is of minor importance.

The Bureau will, in general, test material in a green state, and for this purpose it will be necessary in some cases to bring the sticks artificially to the degree of moisture corresponding to the green state. A sufficient number of sticks will also be tested, after a thorough drying out, to determine the relation between the values in the green state and in the dry state. This information, together with the knowledge of the scientific law governing the relation between moisture and strength, unaffected by defects of checking or unequal moisture dis-

Mr. Hatt. tribution in beams, will be sufficient to enable the designer to decide upon the reducing factors appropriate to the conditions of service under which the timber is to be used. This condition of service involves, usually, either entirely dry timber or timber approximately green.

In all cases, however, the moisture content of the timber will be determined by cutting out discs, about 1 in. thick, from the section of the beam near rupture, and at one of the quarter points, and drying these discs in an oven at a temperature of 100° Fahr. Although this exposure does not determine the entire moisture in a disc, the percentage remaining is less than the error of observation, and the method will determine the necessary empirical reducing factors. It has the further merit that the degree of seasoning of any particular timber can be easily determined by any engineer. A study of the merit of a kiln temperature of 80° is under way. This temperature avoids a disturbance of results due to melting of resin, oxidation, etc. Information will also be procured as to the degree of seasoning to be expected in timber as found on the market. A great deal of timber sold as air-dried in the yards is only dry on the surface, and is practically green in the interior portions.

Species to be Tested.

The species to be tested for mechanical properties should include timber at present in use for which unit values for design are needed, and also timbers which, though available, are not at present in extended use. Of the former, the following species may be noted: Western Red Fir, Hemlock, Spruce; Eastern Spruce, Hemlock and Fir. The western forests will form future supply grounds for the greater part of the timber that will be used in construction. The Red Fir, otherwise called Douglas Spruce or Oregon Pine, has been tested to some extent, both in the former Government tests and by William Hood, Professor H. T. Bovey, Professor C. B. Wing, and Professor Frank Soulé. The Western Hemlock is cut to a large extent and sold on the market as Fir. Both the Hemlock and Spruce are used in filling orders for second-grade Red Fir. The Western Hemlock is a very good timber for constructional purposes, and is also used in inside finishing. It does not shake or splinter. It also possesses immunity against the attacks of insects. It suffers in public estimation from the reputation of the Eastern Hemlock. It grows very rapidly, and may be counted upon to reforest cut-over lands. It has been found of a diameter of 8 ft. and a height of 250 ft. As a rule, the mature trees are from 3 to 5 ft. in diameter, breast high. The Western Spruce is also found in trees of large sizes, and shares with Hemlock an important role in future timber supplies. Considerable supplies of Eastern Spruce, Hemlock and Fir are yet to be found in

Pennsylvania and New York, and furnish constructional material for many purposes in the Northeastern States. A series of tests will be made with large-sized sticks of Southern Pine, to add to the rather meager tests on large-sized Southern Pine sticks, and on the poorer grades of these. Mr. Hatt.

Of the timbers not at present in use for structural purposes, there are large supplies of Southern Red Gum which will furnish timbers of sizes used in building construction. After the better material for flooring, wagon stock, etc., has been sawed, the heart cull will furnish valuable joists. It is proposed, by a series of tests, to determine the quality of these, as well as the properties of the better parts of the log.

Other timbers suitable for rapid growth under extreme conditions of wet and dry sites, as Catalpa and Eucalyptus trees, which are being planted very largely throughout the West and Southwest, should be studied. In Australia the Eucalyptus is one of the most important sources of timber supply for building ships, bridges, agricultural implements, etc.* The Tamarack is a tree suitable for growth in wet situations.

These studies should be on trees from the forest as well as on market timber.

Number of Tests to be Made, and Sizes to Test.

The number of tests depends on the uniformity in the timber of each species and upon the end in view. If it is desired to obtain the strength values for merchantable timber, it would be probably necessary to test, say thirty sticks of each particular size of each special grade; thus thirty stringers of first-class merchantable Red Fir would be tested, and the same number of second-grade merchantable; twenty select stringers and twenty clear stringers. This should be repeated in the case of three or four markets.

If it is desired to study the effect of forest conditions on the strength of the fiber of a definite part of a tree, or the effect of variations in the testing process, the timber selected should contain as few variables as possible, and the number of tests would naturally be arranged to correspond with the quality of timber obtainable. In case of these scientific tests, in which the sizes must be chosen so as to eliminate, as far as possible, other variables than the one under examination, it will be necessary to use sizes of timber 3 x 3 ins. in cross-section, or thereabouts.

In case of tests for the purpose of obtaining unit values, actual sizes should in all cases be tested, as, for instance, car-sill sizes, stringer sizes, roof-truss sizes, carriage stock, columns, floor beams,

* Forestry Bulletin No. 35, page 35.

Mr. Hatt. etc. The appropriate sizes would be quite different in the cases of Red Fir and Hickory.

Quantities and Properties to be Determined.

Some or all of the following properties will be measured, as is appropriate to the use of the species:

Mechanical properties of species.

1.—Strength,
2.—Stiffness,
3.—Resilience,
4.—Hardness,
5.—Brittleness,
6.—Penetration of definite-sized punch,
7.—Abrasive qualities,
8.—Turning qualities,
9.—Weight per cubic foot.

Physical properties.

1.—Shrinkage and swelling and absorptiveness,
2.—Identification by structure,
3.– Identification by appearance of sawed lumber.

The foregoing mechanical and physical properties have partly been determined in the former timber tests. The properties of brittleness and penetration under pressure will be investigated by proper tests. The abrasive qualities have a bearing on the use of wood for paving blocks. In many cases the turning qualities, under a tool in the lathe, must be known. Under the physical properties, the shrinkage is important, from the point of view of the manufacturer. Engineers would be glad to be able to determine the species of sawed timber from its appearance in the lumber yard.

There is at present no standard table of weights of lumber for use in settling disputes between manufacturers and transportation companies, and an endeavor to formulate such a table will be made.

Kinds of Tests.

The kinds of tests to be made in determining these properties cited will include a part or all of the following:

Bending.—

Fiber stress at elastic limit,
Modulus of rupture,
Modulus of elasticity,
Modulus of elastic resilience,
Modulus of ultimate resilience,
Maximum horizontal shearing stress.

Mr. Hatt.

Crushing.—
End of grain; strength, modulus of elasticity,
Side of grain; yield point.

Shearing.—
Radial, transverse.

Torsion.—

Column Tests.—

Hardness.—
By the width of scratch made by a prescribed tool under a prescribed pressure,
By the width of indentation of a prescribed steel cylinder.

Abrasive Qualities.—
By Dorrey abrasion machine, in which the wood is ground by an abrasive powder.

Turning Qualities.—
A piece is put in the lathe and the result photographed.

Brittleness.—
Test beams, under impact tests, 3 x 3 x 36 ins., in size,
Test prisms, under compressional impact, 3 x 3 x 36 ins., size.

Shrinkage.—
Specimen, about 3 x 2 x 12 ins., with sides parallel to radial and transverse direction.

The details of tests which are more interesting to experts in the technique of testing operations, will be presented for discussion before the American Society for Testing Materials at the meeting at Delaware Water Gap, July 1st to 4th, 1903. Some general considerations may be mentioned here.

Methods of Making Shearing Tests.—Various methods of determining the shearing strength have been used. The difficulty is to obtain a pure shear unaccompanied by splitting of the test piece due to bending. This difficulty is often met in case of double shear under tension test. The test piece and method of test are more simple in compression than in tension. Compression tests, apparently, can only be applied properly for the determination of single shear. The latter is more satisfactory, in that the failure occurs on only one surface, and the exact stress may be determined. The shearing tests will be made in compression in single shear. The values obtained by shearing tests on small pieces should be modified by the results of the tests on large beams, for which the shearing stress at the neutral axis should be calculated.

Method of Loading.—The measurement for deflections is most simple when the load is applied at the center of a beam. On the other hand, a larger portion of the stick is brought under the maxi-

Mr. Hatt. mum stress when the load is applied at two points. In the latter case, a longer beam is necessary, in order to yield the proper ratio between depth and span. In the speaker's opinion, the simplest method is to apply the load in the center and compute the bending moment at the point of failure. The loads are to be applied continuously at a fixed speed.

Method of Making Cross-Crushing Tests.—The former Government tests were made on sticks of definite section, and the results give the loads which caused compressions of 3 and 15%, respectively. These results have little significance for other sizes of timber. The method planned will be to apply loads in increments across the stick, and measure the accompanying deflections of the piece, in order to determine the load at the yield point of the timber under this load. The tests will be supplemented by what may be termed penetration tests, in which the load required to bring about the penetration of a punch of given area to a given depth will be determined.

Method of Making Impact Tests.—In determining the relative brittleness of different timbers, the test pieces in compression and bending, both under transverse and actual loads, will be used. In case of compression tests, the test piece, 3 x 3 x 6 ins., will receive blows of increasing height, and the height of blow at which definite failure occurs will be noted. The indication of failure will be the wrinkling of the surface due to the initiation of a shear. In the bending test, the energy required to rupture a 3 x 3 x 36-in. stick will be measured, and also the height of blow required to produce a set of specified amount. These measurements will be made on a revolving drum which receives a record from a pencil attached to the hammer. A study of the resilience of hardwoods is planned.

Organization.

The organization of the timber tests presupposes a central laboratory at Washington, D. C., directed by the Bureau of Forestry, and other testing stations at such points as the Pacific Coast, the Mississippi Valley hardwood centers, and the North Atlantic region. These testing stations will be under the direction and inspection of the central laboratory, to which copies of all data taken will be sent. All tests will be made under uniform methods prescribed in advance. At each testing station there will be a resident engineer with proper assistants to carry on the work. The timber for tests will be selected by the engineer acting in conjunction with a dendrologist and an experienced inspector.

Publication.

In the publications, the results of tests on individual sticks will be quoted, in all cases, and the data presented in such a way that the user

can determine the range of results from which averages are obtained. The data will be marshalled in the form of tables to suit the requirements of users. Mr. Hatt.

SUMMARY.

To summarize the work as planned at present, it may be said that the following series are under consideration, with a view to prosecution.

TESTS TO DETERMINE PROPERTIES OF STRUCTURAL TIMBER.

Series I.—Tests of the Mechanical and Physical Properties of Timber, as Found on the Market.—Material will be of actual sizes and grades of commercial products. The purpose is to determine moduli for design; to determine the value of woods now considered inferior; to determine the liability to knots, and the reducing factors due to these; to arrange a table of standard weights, and rules of inspection and grading; partly to compare the properties of species from different regions.

TESTS TO DETERMINE THE EFFECTS OF VARIATIONS IN THE TESTING PROCESS.

Series II.—Effect of Rate of Application of Load, Including Impact tests.

Series III.—Effect of Moisture.

STUDIES OF THE EFFECT AND EFFICIENCY OF TECHNOLOGICAL PROCESSES.

Series IV.—Preservatives.

Series V.—Methods of Seasoning.

Series VI.—Fire Retardants.

FOR FUTURE DISPOSAL.

Series VII.—Effect of Forest Conditions.

HERMANN VON SCHRENK, Esq. (by letter).—With the increasing use of timber, preserved in one way or another against decay and fire, one of the most important problems which has arisen of late deals with the effect which the preserving process has upon the strength of the preserved timber. Many complaints have been made, and these seem to indicate that, for one reason or another, preserved timber has not the strength which the same material had before being treated with a preservative. This has been true, particularly, of creosoted timber. Many engineers believe, from actual experience, that creosoted timber is likely to be more brittle and less capable of withstanding strains than the same timber before being treated with creosote. This is particularly true with bridge timbers and piling. Mr. von Schrenk.

It has been an exceedingly difficult matter to determine, without actual test, what relationship existed between the preservative proc-

Mr. von Schrenk.

ess and the supposed weakening of the timber. Most of the tests hitherto made with preserved timber have had little actual value. They were made, in the majority of cases, in the following way: Sticks of untreated timber were tested, and similar sticks of treated timber were likewise tested, and the results were compared. In many instances, these turned out in favor of the untreated timber. The reason why such tests are unfair to the preservative is that in the process of preservation, one deals not only with the actual process of impregnation with a preserving substance, but, also, in the majority of treating plants in the United States, with preliminary processes of steam-seasoning. It is very possible to steam a piece of timber, subsequently treated with creosote, to such an extent that the timber becomes exceedingly brittle. This, obviously, will be the fault of the steaming, and not of the creosote. The Bureau of Forestry, of the United States Department of Agriculture, is now planning a comprehensive series of tests to determine the strength of timbers used under various conditions, and one of the series of tests planned deals with the effect of preservatives on the strength and durability of timber. While it is not possible at this time to present a detailed plan for such a series of tests, it may not be without interest to the members of this Society to have a brief outline, presented for discussion at this time, as to what the writer believes ought to be some of the points to be taken into consideration in such a series of tests.

Timber preservation, as stated above, may be divided broadly into three stages: First, the preliminary preparation; second, the actual preservative process; and third, the treatment of timber following preservation. In each of these stages the timber is subjected to treatment of one kind or another, and the final result, as to strength, is influenced materially by each of the stages.

Preliminary Preparation.

The preliminary preparation of timber for preservation must take into account the following factors:

1. *The Time At Which the Timber is Cut.*—The strength of timber differs materially, depending upon whether it is cut in winter, spring or summer. Any test, as to the influence of a preservative, should be made in such a way that the untreated and treated sticks are cut as nearly as possible at the same season of the year, so as to eliminate the seasonal factor.

2. *Seasoning.*—Before the actual preservative process, the timber is generally seasoned by one of several methods. The method commonly used in Europe is to air-season it. In some plants kiln-drying is resorted to. In the United States, most of the timber-treating plants use a process of steam-seasoning. Careful tests should take into account the influence, particularly of kiln- and steam-seasoning,

on preservation. One of the first series of tests in connection with timber preservation should relate to the influence of the following factors, in connection with steam-seasoning in particular. First, a test should be made to determine the influence of subjecting timber in the treating cylinder to steaming for various lengths of time with live steam. Second, a similar series should seek to determine the influence of steaming at various temperatures, and this will include steaming under pressure. Third, a similar series should determine the influence of steaming, for various lengths of time and for various temperatures, with superheated steam in closed coils. Mr. von Schrenk.

The tests with steaming are considered of prime importance, because it is believed that much of the supposed weakening of timber by preservative processes is due to improper and excessive subjection of timber to live steam in the treating cylinder.

Preservative Processes.

Tests as to the direct influence of the preservative on the strength of various classes of timber should be conducted, as far as possible, with the exclusion of such factors as have been mentioned above. In other words, in testing the influence of zinc chloride on timber, all timbers tested should be subjected to a similar amount of steaming, kiln-drying or air-seasoning, so that the only factor to be considered would be the impregnation with the chloride. Tests to determine the influence of preservatives on the timber should consider the use of the preservative in varying quantities, for it is entirely possible that a preservative used up to a certain strength does not weaken the timber, while excessive use may weaken it considerably. The following preservative processes should be included in any series of tests:

1st.—Zinc chloride.

2d. —Creosote or tar-oil, and its various forms, such as spiritine, carbolineum, etc.

3d. —A combination of zinc chloride and creosote.

4th.—The Hassellmann or Barschall treatment. This process will involve the additional testing as to the influence of boiling timber in solutions at various temperatures.

5th.—The electrical process of timber treatment, using magnesium sulphate.

6th.—The Ferrell process, using various salts.

7th.—The creo-resinate process.

In making comparative-strength tests of treated and untreated timbers, it will be very desirable to eliminate, as far as possible, the variable strength factor found in untreated wood. To this end, the method used during the last year has been found very effective. The given log was sawed into two pieces, one half was treated, and the other half left untreated. The results, as to strength obtained after the two pieces were tested, eliminated this variable factor almost

Mr. von Schrenk. entirely, because the two pieces came from the same tree, and from about the same region in its trunk.

Subsequent Treatment.

The influence of drying or seasoning after treatment, on the strength of timber, has not been fully realized. It is believed that kiln- or air-drying treated timber will in many instances increase the strength of the material. A series of tests should be made in order to determine to what extent subsequent drying of treated timbers affects the strength of such material.

Several experimental plants are now under process of construction, where it will be possible to measure accurately the time during which timber is steamed or impregnated, the temperatures at which steaming or impregnation takes place, and also the pressure. Accurate scales are being fitted up to weigh large timbers, both before and after treatment, to determine the loss or gain in weight. It is hoped that the Society will discuss this phase of the timber-testing question very fully. Any suggestions which the members may have, referring to the foregoing general outline, or any new phases which it would seem desirable to consider, ought to be brought forward. The foregoing outline is simply a general statement, which is to be worked out in detail in the near future.

Mr. Lanza. Gaetano Lanza, Esq. (by letter).—At the request of Mr. Pinchot, a proposed general outline for the tests of timber to be made by the Division of Forestry, of the Department of Agriculture, was submitted to him on June 23d, 1902.

This general outline is quoted in their printed circular, beginning on page 3, and ending on page 8, and will not be repeated here; but the following topics will be discussed, viz.:

1.—Some general remarks on testing timber, including the importance of making the tests upon such sizes, and such kinds of timber, as are used by, or are useful for, engineers, and under the conditions of practice, especially as regards knots, cracks, crooked grain, and other defects.

2.—Criticism of the work performed upon this subject by the Division of Forestry, between 1891 and 1896, which was published in "Timber Physics," Volume II.

3.—The value and importance of the tests proposed in Series I, page 1, of the circular.

4.—Remarks upon certain portions of the circular.

1.—It is evident to every engineer that the timber beams used in practice are very rarely 2 x 2 ins. in section, that 4 x 12 ins., 6 x 12 ins., 7 x 16 ins., and larger, are sizes in common use, and that these latter may properly be called full-sized pieces.

It is also plain, to anyone who uses timber in building, that full-sized pieces contain knots, cracks, crooked grain, and other defects, which would not be tolerated in the 2 x 2-in. pieces which, in the olden time, were used so extensively for tests, the results of which, as is well known to-day, do not furnish any constants suitable to use in designing timber structures. Mr. Lanza.

Indeed, for any one kind of timber, the greatest influence upon its resisting powers is exerted by the defects stated, and their effect is even greater than that of the percentage of moisture, or degree of seasoning, though this latter ranks next in importance.

Anyone who has tested a considerable number of full-sized timber beams, and has attempted to estimate beforehand their strength and compare his estimate with the facts, will be thoroughly convinced of the truth of the statements made above.

Indeed, the writer knows, from personal experience, in the case of Spruce, that a careful consideration of the knots, cracks, crooked grain, and other visible defects will enable one who takes pains to keep himself in practice to predict the breaking strength of such a 4 x 12-in. beam within about 5%, the degree of dryness being considered only to such an extent as can be judged by the eye.

In the light of the above, it is plain that any experimental study of the resisting properties of timber which does not take these things into account is wholly unsuited to furnish such information as the engineer needs. Hence the only tests from which we can hope to derive information of value are those on full-sized pieces.

In making such tests, we are brought, at once, to a realization of the fact that there is a great variation between different pieces of the same lot, all suitable for use, the strongest being often three times as strong as the weakest. In practice, therefore, when, as is usual at the present time, the entire lot is used indiscriminately, the constants used in designing should be very considerably below the average values.

If the time should ever come when the engineer makes a selection between different beams or columns of the same kind of timber, that selection will be made mainly upon the basis of the knots, cracks, crooked grain, and other defects, while the determination of the actual strength will also involve, to some extent, a consideration of the percentage of moisture.

2.—As to the report of the work done upon Long-Leaf Yellow Pine, under the auspices of the Division of Forestry, between 1891 and 1896, the results of which were published in "Timber Physics," Volume II, the following observations should be made:

a.—By far the greater part of the work was performed upon small pieces, and the attempt was made to imitate that portion of the Munich tests which was made upon small pieces. Inasmuch as these results do not furnish values suitable for use, they will not be discussed here.

Mr. Lanza. *b.*—The only results published in that volume which were obtained from tests of full-sized pieces may be said to be of twenty-five beams from nine trees, all from one locality, if we call 4 x 8-in. pieces full-sized; otherwise they include only seven beams from four trees.

The number of tests and of trees was wholly inadequate to justify the drawing of conclusions of value to the engineer, even if the tests had been made correctly.

c.—The testing machine used did not weigh correctly, on account of the friction of the cup-leather packings; and, whereas, after this had been criticized severely, Professor Johnson changed the construction of the machine to that shown in his book, "Materials of Construction," nevertheless, the results on full-sized pieces were all obtained with the incorrect machine.

d.—Pains were not taken, in the determination of the moisture, to make sure that the percentage contained in the pieces tested for this purpose was really the average of that in the entire beam.

e.—Therefore, it seems to the writer that the results of the tests published in "Timber Physics," Volume II, do not furnish such information as is needed by the engineer, and that, therefore, in the new tests, advantage cannot be taken of any of those results.

f.—In Circular No. 15, of the Forestry Division, no details are given, and hence it is presumable that the tests there reported were made in a manner similar to those reported in "Timber Physics," Volume II.

3.—Tests of the character described on pages 2 and 9 of the circular, under the heading Series I, are of very great importance, as they will serve to furnish quickly the information most needed by engineers, *i. e.*, the constants (compressive strength, modulus of rupture, and modulus of elasticity) that can be depended upon in any one kind of timber. For this purpose, of course, pieces of various qualities should be selected, including some of the poorest that would be suitable to use in construction. Indeed, the above is the character of the greater part of the tests on full-sized timber that have been made in the writer's laboratory, especially upon Spruce, Yellow Pine, and White Oak, as well as upon a few other kinds.

In the case of timber bought at the lumber yards, very little information, if any, can be obtained regarding its growth, seasoning, etc.; but, even without such information, we can obtain values for ready use for a number of kinds of wood, which would serve a very useful purpose.

4.—The writer will now discuss a few of the matters which are referred to in the circular, and which need some comment.

a. The circular does not contain any proposition for the making of time tests upon full-sized beams, a matter of very great importance. Tests of this character should be made at every station, in order to determine the effect upon all the constants, of subjecting the pieces to

load for a long period. In the writer's laboratory, such tests have been carried on for periods varying from about five months to about three years. Mr. Lanza.

b.—The two schemes for moisture determination, mentioned in the writer's outline, are not adapted to the separation of the volatile oils from the moisture, a matter of much importance.

The method proposed in the circular, for separating the two, will be a proper one, provided it can be shown that the three discs chosen in the manner described represent fairly the percentage of moisture in the entire beam.

The only way to settle this question is to make a preliminary investigation, to determine how (if at all) any discs can be chosen which will represent fairly the quality of the piece as to moisture. If the result of such an investigation should show (as the writer is inclined to think) that such discs do not represent fairly the entire beam, then a proper method to be pursued would be the following:

Determine the percentage of volatile oils in a set of three discs.

Determine the percentage of volatile matter in a set of three adjacent discs.

Determine the percentage of volatile matter in the beam from the remainder of the beam (*i. e.*, all except the six discs mentioned above), by the first of the two methods given in the writer's outline.

Determine the percentage of volatile oils in the beam by proportion.

c.—The tests for compression across the grain should be much more extensive than those proposed in the circular, in order to furnish information of value. They should include a considerable range of ratio of length to diameter of the pieces to be tested.

d.—It may be well to mention a few minor matters, as follows:

α.—The writer does not believe that the yield point of timber, even if it exists, can be found with sufficient accuracy to make the results of any value.

β.—From the transverse breaking load, the modulus of rupture can be calculated, but not the extreme fiber stress at fracture.

γ.—It seems to the writer at least doubtful whether deflections can be obtained with sufficient accuracy, if the load is to be increased continuously.

*Referring to Mr. Johnson's remarks in reply to the writer's, regarding the former timber tests of the Division of Forestry, no comments will be made under the headings (*a*), (*e*) or (*f*), as it would seem that they have been fully answered in the remarks of Professor Hatt.

Having already stated that an experimental investigation is needed, in order to ascertain the best method of determining the percentage of moisture, no further comment will be made on (*d*).

* These remarks added by Professor Lanza after reading Mr. Johnson's additional discussion.

Mr. Lanza. In regard to (*b*) and (*c*) the remark will be made, that it was in September, 1895, that the writer was informed verbally by Professor J. B. Johnson, that, in consequence of criticisms by the writer, and presumably by others, he (Professor Johnson) had been led to recalibrate his machine, and, finding it in error, had made the change mentioned by Mr. A. L. Johnson, for the purpose of avoiding packing friction. Indeed, the change does not appear to have been made until some time after making the tests which were reported in Timber Physics, Vol. II, published in 1893.

Mr. Johnson. A. L. JOHNSON, M. Am. Soc. C. E.—While the speaker has had very little to do with timber since 1895, except in the way of false-work construction, his former connection with the Government tests, during a period of two and one-half years, aroused in him an interest in timber physics which has not yet, and he trusts will never be, abated.

In the old days, we had much enthusiasm, but little money. Now, the speaker understands, they have much enthusiasm and much money. No more auspicious circumstances could be devised, and great results may be expected. In the spring of 1895 there were some discussions relative to the formulation of a broad and completely comprehensive scheme, embracing the whole field of timber investigation, but, before anything had been done along this line, the appropriation ran out, and the speaker's connection with the Division of Forestry ceased; and, as far as he knows, no small-scale drawing of the whole field has ever been made.

Yet this is absolutely essential, not to the end that we may begin at the beginning and carry everything before us as we go, but for exactly the opposite reason; so that we may make investigations here and there, as exigencies may demand, or as suits our convenience, feeling comfortable in the assurance that each step made leaves one less to make, whatever the order in which taken. In the making of apparently entirely disconnected investigations, the fields, nevertheless, will overlap, and these portions will be done twice, and perhaps many times.

In the speaker's estimation, therefore, the thing to be done now is for the Forestry Bureau to prepare an outline scheme of all possible lines of investigation which, by our present knowledge, would indicate any profitable return. No doubt the Bureau would be glad to receive suggestions for such a scheme, and the speaker has the temerity to present one herewith for consideration. He does not regard it as satisfactory; in fact, it is extremely improbable that a satisfactory scheme could be prepared by any one man; and certainly none should be adopted except after extensive discussion by technical experts.

With a general plan for the whole work, the Bureau would then be in a position to receive suggestions from engineers, architects, manufacturers and others as to the necessity for immediate information along certain lines.

When we begin to consider the whole field of timber investigation, about the first thing we realize is, that engineers are not the only ones using timber, though, undoubtedly, they are the most strenuous in their demands. First of all, then, it becomes necessary to classify the uses of timber, and the speaker suggests the following segregation: Mr. Johnson

USES OF TIMBER.

Utilitarian.

Framed structures.
Piles.
Ties.
Tanks.
Walks.
Boxes, cases, etc.
Implements, tools, utensils, etc.
Instruments.
Vehicles.
By-products.

Æsthetic.

Finish.
Furniture.
Musical instruments.

With regard to the classification of the headings in the investigation scheme given in Table No. 1, a few words of introduction may not be out of place.

The eventuality hoped for is that we, or rather our children's grand-children, may be able to grow timber for a given purpose. This naturally suggests two main divisions in the investigation, one to determine the best uses of the timber we have, the other to find out how to produce what we would like to have. The first the speaker calls "Utilization," the second "Cultivation." These divisions are shown in Table No. 1; the sub-headings are self-explanatory.

Of the "Utilization" part of this scheme, practically the whole value will be summed up in the commercial consideration as to the most suitable timber and treatment for different purposes. The value of the second part is comprised in the determination of the relation between site conditions and anatomical structure, enabling us to locate forests where the trees will develop the desired qualities if properly cared for.

As may be inferred from the foregoing, the speaker does not approve of Series No. 1 of the scheme proposed by the Bureau, inasmuch as all tests made on timber bought in the open market will be wholly lost as far as the correlation with site conditions is concerned. This he deems to be a waste of time and money, not offset by any im-

Mr. Johnson. mediate advantage. In many cases the Bureau officials, themselves, would be unable to tell what kind of timber was being tested.

TABLE No. 1.

TIMBER PHYSICS.—GENERAL SCHEME OF INVESTIGATION.

UTILIZATION.

Physical Considerations.

Size of log.	Cross-breaking strength.
Age of log.	Crushing strength, endwise.
Size of test piece.	Crushing strength, across grain.
Rate of growth (average).	Penetration.
Position in cross-section.	Tensile strength.
Position above ground.	Shearing strength.
Seasoning processes.	Resilience.
Preserving processes.	Load with no initial velocity.
Artificial compositions.	Load with initial velocity.
Fire retardants.	Resonance.
Moisture.	Polish.
Oils.	Finish.
Defects.	Paint.
Liability to diseases.	Stain.
	Varnish.
	Shrinkage or swelling.
	Weight.
	Length of life.
	Chemical qualities.

Commercial Considerations.

Cost.
Present and prospective supply.
Reliability or uniformity of grade.
Characteristics by which wood may be identified.
Rules of inspection.
Relative suitability of different woods and treatments for different purposes.

CULTIVATION.

Botanical Considerations.

Anatomical structure correlated with { Physical properties. / Site conditions.

Commercial Considerations.

Placing forests for most suitable site conditions.
Directing the use of timber so as to avoid needless waste.
Apportioning forests according to needs.
Harvesting for perpetual service.

Series II to VI are necessary, but should all be made on material whose site conditions are known. A series on fire retardants might also be profitably added. Mr. Johnson

With regard to Series VI, the speaker, just before leaving the Forestry Division, had planned an investigation which it was thought would give the resilience of the material under loads applied with initial velocity. It was at that time considered impracticable to measure the energy of the blow directly, on account of the large number of ways in which the effect of the blow was dissipated, rendering it impossible to determine just how much was absorbed by the specimen itself.

It was proposed to turn rods, say 1 in. in diameter and 3 ft. long, rounded at the ends, all at the same condition of moisture, and, striking them on the end with a hammer, determine the pitch of the note produced. This note results from waves set up within the piece, and its pitch really represents the rate of transmission of stress through the specimen. A wood giving a high note would undoubtedly have a great impact resilience. Incidentally, this series might have been of considerable value to manufacturers of musical instruments.

Series VI should be made to include constant loads. An investigation of this sort was made in the former tests, whereby it was found that one-half the load required to break a beam in a few minutes would do so in an infinite time. But the series was very limited in scope, it being started only a short while before the work was stopped.

With regard to the old series of tests, the speaker believes they may be taken as conclusive, as far as the relative strength of *Pinus Palustris*, *Pinus Echinata*, *Pinus Cubensis* and *Pinus Tæda* are concerned; but not conclusive with regard to the other woods, nor as to methods of treating timber; except as to the bleeding of the Pines.

The details of execution of the Bureau's proposed scheme leaves little to be desired. In the speaker's opinion, however, there are one or two points needing more careful handling.

The method of obtaining the moisture contents by cross-section disks, although used in the first tests, and while fairly satisfactory for all kinds of tests except on beams, is not satisfactory for the latter. Especially would this be true of large beams, or beams of varying size. The reason for this is that the interior of the beam usually contains more moisture than the outside, so that the ordinary stress-strain curve of the timber is modified by the varying influence of the moisture contents. The average moisture of the section, therefore, is likely to be much in error, as far as the influential moisture contents near the surface are concerned. This would make it difficult to compare results on large and small specimens. The large beams would show greater strength, relatively, than they really would have.

A satisfactory comparison could probably be obtained if the beams

Mr. Johnson. were all made of the same size, with a separate series to determine the influence of size, as was done in the first investigation. But a better method of obtaining moisture contents should be devised. The speaker believes this can be done by cutting the disks in slices parallel to the neutral axis and giving weight to the different percentages in the different pieces in proportion to their distance from the neutral plane. At any rate, this should be done until it is determined that the method alters the results too slightly to be worth the trouble. But a very slight change in moisture makes a large change in stress at the low percentage for kiln-dried material, and the speaker believes there will be a considerable difference in results shown by this method.

If, for beams, the old disk method is poor, the method of obtaining the moisture contents by weighing the whole stick is very much worse. The moisture in the ends of a beam cuts practically no figure in the result, but would, by such a method, be given as much weight as that in the middle.

The Bureau, however, has not seriously considered the adoption of the above suggestion.

The speaker would counsel small minor tests and many of them, making size, with associated defects, a separate investigation, as shown in his scheme. The reason for this is that, do what we will, there are many more unknown quantities than equations in such work, so that results can only be obtained by a large number of tests. If we increase the number of variables we must increase the number of tests in an, at least, equal proportion.

To use, as a minimum unit, beams 4 x 12 ins. x 14 ft. would about double the variables, as compared with what they were with the original beams, 4 x 4 ins. x 6 ft.; hence it would take about twice as many of the larger beams as of the smaller ones to be able to draw scientific conclusions of equal value regarding the strength of different species. In other words, it would take fourteen times as much timber for the large tests as for the smaller ones, to obtain the same accuracy.

The circular states that it is proposed to load the beams in the middle. This is a poor method for anything but the most homogeneous material. It compels a break at a certain point, whereas the desire may be to break at a different point. The results, therefore, are too high. This was all threshed out in the original investigation, and it was found much better to have a uniform bending moment for some distance (12 ins. was used) in the middle.

The speaker, not being at present a user of much timber, will leave it to those who are to suggest the important parts of the general scheme requiring immediate investigation. But there is one suggestion he would like to make to the Bureau, and that is that they pre-

pare a handbook, of pocket size, for the identification of timber, and of trees in both summer and winter. Mr. Johnson.

Every engineer would feel the value of such a book. Of what service to him is it for the Department to say that one kind of wood is much better than another for certain purposes, when he is unable to tell whether or not he gets it ?

The speaker remembers some of the results on shear in beams obtained in the first investigation. Some of the largest beams failed by shearing along the neutral plane, having become case-hardened, as they say, by drying too rapidly and hence checking badly; being a case which is likely to occur very often in practice. They were of Long-leaf Pine, and failed at about 150 to 200 lbs. per square inch, whereas the small-sized shearing specimens of that wood would have shown a shearing strength of from 600 to 700 lbs. per square inch; and the tables which were prepared by the Forestry Bureau, in Bulletin No. 12, cautioned a much greater factor of safety in big beams than in small beams, on that account.

The speaker would never design a large beam to take more than about 100 lbs. per square inch of shearing stress along the neutral plane. This was one reason why, in the Bureau's design of timber-trestle bridges, corbels were used, as the bolts running through the corbels at the ends of the stringer would assist it in increasing the shearing strength at that point.

*Referring to Professor Lanza's statement in regard to the former timber tests, the writer submits the following:

a.—By far the greater portion of the work was, as he says, performed upon small pieces, which he rejects as being of no service, as the values are not representative of practical conditions. The writer would take issue with him upon this point, the respective positions being arrived at from totally different points of view concerning the functions of the United States Government in work of such character. The writer's position is, that the United States Government would not be warranted in spending money to determine the strength qualities of ordinary merchant timber for the comparatively few people who would be interested in the results. To obtain these factors alone involves no particular difficulty, and no work of such a high grade as could not be found in the engineering force of any railway system of the United States.

The real problem of timber investigation is the correlation of physical qualities with site conditions, in order to know how to grow a given timber for its most satisfactory uses. Such a problem involves a thorough knowledge of engineering, botany and forestry, thousands of tests, and years of study, but has in view a benefit to the majority

*This part of Mr. Johnson's discussion was written after reading the discussions by Messrs. Hatt, von Schrenk, Lanza and Russell.

Mr. Johnson. of the citizens of the United States. Such, only, is the character of investigation warranted on the part of the United States Government. But, with a scheme prepared along such lines, it would easily be possible to make isolated investigations designed for the particular benefit of a limited number of people, the results being taken in such a way as would enable them to form a part of the general scheme. Accepting this view, it is seen at once that the number of variables must be reduced to a minimum in any given piece, because the values of the different variables must be known. For example, while the first beam test might be made on a large timber, it would then be necessary to cut up this piece into many small pieces, and ascertain the influences of size, defects and location in the tree.

b and c.—Professor Lanza states that the large beams tested in the first investigation were too few in number to warrant broad or reliable conclusions, especially as the weighing device of the beam machine was inaccurate.

The writer was in charge of the work at the laboratory, and most of these tests were made by him. The beam had been calibrated by a set of helical springs, and the weighing was done on the first few beams, at some distance away, on a 100 000-lb. Riehlé machine; but, as it was very quickly discovered that there was considerable friction in the packing, which might lead to erroneous results, a platform scale was inserted under one end, and most of the beams were tested in this manner, in which there could be no doubt as to the accuracy of the load.

Of course, it is true that, except for the cost, it would have been very desirable to have had a greater number of full-sized beams tested, and it had always been a part of the general scheme of investigation, but, at the time, sufficient money was not available.

d.—Professor Lanza here says that:

"Pains were not taken, in the determination of the moisture, to make sure that the percentage contained in the pieces tested for this purpose was really the average of that in the entire beam."

If the moisture in the pieces tested had represented the average for the entire beam, no indication whatever could have been obtained as to the influence of moisture on the strength of the piece. In such beams there is moisture which has an influence on the strength, and moisture which has no influence on the strength, and by far the larger percentage of moisture in the beam is of the latter character. The influential moisture is the percentage contained adjacent to the points of maximum stress, which are in the extreme fibers at the sections of maximum moment. The writer has heretofore suggested a method of obtaining the moisture for the large beams, which method would give the greatest disagreement with the average percentage, and yet, in his estimation, far more reliable results, as to the true influence of the moisture, than the scheme suggested by Professor Lanza.

e.—Naturally, the writer does not agree with Professor Lanza when he states that it would not be advisable, for the new series of tests, to accept the data hitherto published, but that it will be necessary to cover this ground again. Mr. Johnson.

All the tests, except those on a few of the large beams to which reference has been made, were made in a most accurate manner, and were planned as a part of a general scheme—not formulated, but held in mind—along the lines of the tables above given; and any of these results can be used to-day or a hundred years from now, as a part of such a broad and comprehensive scheme, all the site conditions of growth being known, and a botanical analysis made of the whole. They are in value, to the results that would be obtained in the Bureau's Series I, as are the astronomical results of a transit of Venus expedition to a boy's observations through smoked glass.

f.—Circular No. 15, of the Forestry Division, was prepared after the writer's connection with the Department ceased, and if the results referred to in this circular were obtained as were those reported in previous publications, they will be found to furnish scientific information of a high order, when a proper study of the subject has been inaugurated.

S. Bent Russell, M. Am. Soc. C. E.—If it is proper to speak of matters of detail, in discussing this subject, the speaker would like to offer one or two suggestions which seem to be practical. Mr. Russell.

In the tables of strength of beams, published since the Government investigation was started, the strength of short beams is given as being independent of the length. That is, when the length is short in comparison to the depth, the central breaking load of the beam would be independent of the length.

It has been shown, of course, that in the case of a beam which fails by shear along the neutral axis, the theoretical breaking load is the same for different lengths when the cross-section is the same. The speaker, however, has seen no experimental data which would be of use in the actual designing of beams of this character. To make the point clear, suppose we have a wooden beam, 16 ins. deep and having a clear span of 6 ft., loaded at the center. How should the breaking load be determined? From the published tables, such a beam would not carry any more than a similar beam of 12 ft. clear span loaded at the center. It seems quite probable, however, that the shorter beam would really sustain a greater load. The actual length of the former beam would be somewhat more than 6 ft., to provide proper end bearings; and the load, in practice, would have to be distributed enough to prevent local crushing. These conditions would almost certainly affect the tendency to shear along the neutral axis. It is not unlikely, too, that the shorter the beam the greater would be the effect of these conditions upon the breaking load.

Mr. Russell. Beams of the kind in question are used to a large extent in engineering construction, and it would certainly be wise to determine the laws that govern their strength. The speaker would suggest that a series of tests be made with properly designed wooden beams where the span is short compared with the depth of the beam, as such a series of tests, properly planned and executed, would be of great commercial value.

Another subject for investigation is the effect of water-soaking upon the strength of timber; the effect of soaking, under different pressures and for different lengths of time, and for different kinds of timber. An engineer often has occasion to use timber under water, and would therefore like to know what the strength of that timber is after a certain time of immersion. The data now obtainable are by no means sufficient. It is true that tests have been made of the effect of percentage contents of moisture upon the strength of certain kinds of timber, but that is not enough. The speaker would suggest that an attempt be made to obtain more data to cover this point.

These suggestions are the fruit of actual problems in the speaker's practice; and, without doubt, many engineers can recall similar problems of their own.

Mr. Hatt. W. K. HATT, Assoc. M. Am. Soc. C. E.—The speaker desires to comment upon certain matters brought up by Mr. Johnson. Touching the relation to the general interests of forestry of tests to determine unit values of the strength and stiffness of timber, it seems to the speaker that any factor affecting the use of timber has an influence upon the work of the forester. For example, if the true value of such a wood as second-growth Loblolly Pine should be determined and its use promoted, the work of the forester in bringing about a conservative management of forests would thereby be greatly aided and the interests of the public promoted.

The outline that the speaker has in mind, of an ideal scheme of investigation of all matters relating to timber, agrees with that presented by Mr. Johnson. The pursuit and successful completion of a scheme of this kind is a very attractive ideal, and, in the speaker's opinion, therefore somewhat dangerous, because impracticable. The investigator would soon find himself in a labyrinth. It is almost certain that this somewhat Utopian scheme would not be carried to a successful conclusion, and in the endeavor to use the same material to supply data for different parts of the general field of investigation the investigator will probably fall between two stools. For instance, the sticks used in construction are taken from a log in a different way from those that are best adapted to yield representative values for the mechanical properties of the tree. Again, the German investigators, in order to determine the effect of minor conditions upon the strength of wood, perform tests on small sticks which do not contain knots

and other accidental defects; whereas, in this country, large sticks are tested for the very reason that they do contain knots. The very object of one series of the proposed investigation is to determine the extent to which these knots and other defects lower the strength of the timber. Mr. Hatt.

With regard to Mr. Johnson's programme, which comprehends all possible relations between growth and strength, it is certain that only a few of the possible conditions of growth have any important influence. A knowledge of the relation of many of the factors listed by Mr. Johnson would be interesting; it is certain that the effect of these factors could not be isolated and determined. A knowledge of many of these would not be of sufficient value to justify the expenditure of the necessary time and money. Roth's study of the Bald Cypress*, in which trees were collected from various sites, brought out the fact that individual variations and variations within one and the same tree are fully as great as the differences brought out in the woods from different localities. Roth's study of the White Pine† also showed that the specific weight (which varies directly with the strength) of wood from swamp trees is no heavier or lighter than the wood from upland trees, the trees from New England differing apparently in no way from those of the Lake region or North Carolina. No useful results, to the writer's knowledge, have followed the attempts which have been made to determine by a series of formal tests the relations which exist between conditions of growth and quality of timber.

Has it not occurred to Mr. Johnson and others that the collector operating in the forest can only describe the conditions which exist at the period of the visit, whereas the tree has been growing in some cases two hundred years? The collector in the market knows more of the history of the tree than does the collector in the forest, for the former sees the history in the annual rings.

Another instance in which one important object of tests of timber is frustrated, by the endeavor to kill two birds with one stone, is the European method of selecting test specimens so that values may be obtained representing the average mechanical quality of the tree. The Germans quarrel with the former Government timber tests because the manner of taking out the test pieces, in their opinion, did not show the average quality of the tree. Now, even if some method were agreed upon for the selection of test pieces that would serve this purpose, such an average quality would have no practical meaning, for the reason that the practical questions to be solved have relation to only part of the wood of the trunk. To obtain mechanical values that shall have a useful significance for the designer or manufacturer, tests should be made upon that portion of the trunk that is actually used. A factor representing the strength of the whole

* Circular No. 19, Division of Forestry.

† Circular No. 22, Division of Forestry.

Mr. Hatt. trunk has but little meaning if desired to determine, for instance, the relative value for carriage construction of second-growth or virgin-growth Hickory, or the relative value of Swamp White Oak or Upland White Oak. The comparisons should be made upon the wood taken from that part of the trunk which is available to the manufacturer for such purposes.

Mr. Johnson will, no doubt, agree that, for the purpose of establishing the mechanical value of structural timber, it is necessary to test large sticks. He believes, however, that these should be taken from the forest, in the interest of general scientific knowledge. In favor of sticks as obtained on the market or at the mill, the speaker would point out the following advantages:

(1) Economy.

(2) The certainty that the material represents an actual commercial product.

(3) That all the essential facts, certainly all those that the constructor can, in any way, control, can be determined with reference to the history of the timber. In case of large structural timber, the lumber dealer can furnish information as to the mill from which the timber was procured, the month in which it was sawed, and the conditions of seasoning. In case of smaller sticks of hardwood, procured at the mill, this information is also available. Knowing the mill, the forest conditions from which the mill draws its supply can easily be determined. A competent dendrologist has no difficulty in determining the species of at least 95% of the timber on the market by an examination of the wood fiber. At any rate, for the purposes of obtaining unit values for the design of framed structures, if two sticks of timber of closely related species, as, for instance, Long-Leaf Pine and Cuban Pine, are so closely alike in weight and appearance of fiber that an expert cannot distinguish between them, the confusion of one species with another is a matter of no practical importance.

The speaker would digress in order to state his personal opinion as to the most economical method of obtaining knowledge of the effect of conditions of growth upon the volume and character of the wood produced in the forest. He believes it to be an unnecessary and rather clumsy method to cut down many trees, and submit them to the action of the testing machine in order to determine the relations sought. As a matter of fact, many practical lumbermen, from general observation and experience, have a qualitative knowledge of the effect of conditions of growth on the value of the wood. This knowledge must always be largely inferential and can never be precise. The mechanical work of the testing machine, in the speaker's view, should establish the relations between specific weight and strength; between moisture and strength; between different charac-

teristics of fiber and strength; and should determine the relations which exist between the moduli of strength exhibited by any species under different kinds of loading, to the end that, from the results of one kind of test, the strength may be predicted under another kind of loading. The testing machine can also determine the mechanical effect of different technological processes on identical material. In this way, in general, the testing machine establishes the relation of certain physical appearances and properties to the strength under loading. The number of cases is not very great in any one species, and it would be a comparatively simple matter to determine these relations. Mr. Hatt.

Now, the botanist and the forester, in connection with lumbering operations, or a study of forests, can easily determine the character of wood produced under different growth conditions. With the information derived from the testing machine at his hand, he can easily infer the mechanical worth of the different kinds of wood produced under given growth conditions. In this way, a single individual in a few weeks in a forest can learn more than an entire organization of testing engineers in many years. This is the speaker's personal view, however, which has not been submitted for criticism to any botanist or forester.

A useful end for timber tests at the present time is the determination of which of the so-called inferior species will prove the best substitutes for those woods whose increasing price is gradually placing them out of reach for certain purposes. This problem involves the determination, in a quantitative measure, of the difference in strength and durability of two woods which are in use for the same purpose, so that an economical selection between these two may be made. This remark applies, for instance, to material for cross-arms for telegraph poles, box boards, joists, etc.

In answer to Mr. Russell's question as to the strength of a short block which is under a bending load, the speaker would say that tests have been made to determine the relation between the strength of sticks of long and short span by Mr. Hood, Chief Engineer of the Southern Pacific Railway. It is known, of course, that large sticks of short span must be designed for horizontal shear, and in extreme cases, possibly, for compression along the side of the grain at the point of application of the load.

There is every reason to believe, from actual tests and general experience, that the effect of soaking timber is to improve its durability, because of the fact that such soaking removes the sap. The process of soaking also renders hardwoods less likely to warp. Of course, if soaked timber is tested immediately after removal from the water, the strength is less than that of dry timber; but there is no reason to believe that, if the timber which has been soaked is subsequently dried, there will be any diminution in the strength due to the previous soaking.

Mr. Hatt. In forming views of the method of procedure for testing timber, one should remember that there are endless possibilities in respect of refinement of methods, and the exercise of judgment is necessary in rejecting useless refinements. One must also choose between those investigations which are of scientific interest and those which have some outcome of practical use. The following are the matters which appear to the speaker to be worthy of investigation at present:

WITH REFERENCE TO INTERNAL RELATIONS OF THE TESTING PROCESS.

(1) The relation between the physical and mechanical properties, so that, in future, from the result of a single test the engineer may compute the strength under the several kinds of loading, or the botanist may judge of the value of woods from inspection.

(2) The effect of moisture and speed of application of the load.

(3) In a quantitative measure, the weakening effect due to knots and defects of structure.

WITH REFERENCE TO KNOWLEDGE OF PROPERTIES OF SPECIES.

(4) To ascertain the value of inferior woods now used in market, as, for instance, second-growth Loblolly Pine; and of species that will come on the market in the future, as, for instance, Western Hemlock and Spruce. This knowledge would allow a proper choice of substitutes for woods now becoming scarce. The work of the forester, in conservative forest management, will be aided thereby.

WITH REFERENCE TO TECHNOLOGICAL PROCESSES.

(5) To study the best methods of seasoning, and the effect of these on the strength and durability.

(6) To study methods of preserving; their efficiency; their effect on strength; and the best species for use.

(7) To work out special problems, such as the determination of sizes of cross-arms of telegraph poles or box boards, in order to economize material.

In answer to Professor Lanza's remarks, the speaker would say that provision is made for "time tests" on full-sized timbers; that preliminary investigations show that the corrections to the moisture determinations due to the volatilization of the oil in the wood are of minor importance, and may be neglected, except in the cases of very fat Long-Leaf Pine. The correction in ordinary is cases a fraction of 1 per cent.

The present state of knowledge would lead to the belief that the variation in the moisture along the beam is small in comparison to the variation throughout the cross-section. The testing of timber in the green condition will avoid many difficulties arising from the effect of

differences of moisture on the strength of the timber, for the reason that after timber has absorbed 33% moisture, no additional weakening effect accompanies any further absorption. A sufficient number of beams are to be tested after a thorough drying out to determine the relations of the strength of dry to green timber. The tests for compression across the grain have been planned to include full-sized pieces, in accordance with the suggestion of Professor Lanza. Mr. Hatt.

According to the speaker's experience, the yield point of timber is a limit of sufficient definiteness to form a proper and useful basis for the designer. It is recognized that the modulus of rupture is not the same as the extreme fiber stress at fracture. In case of large beams, which deflect from 2 to 3 ins., or even more, it is sufficiently accurate to read the deflections to $\frac{1}{100}$ in. This can be done under a continuously increasing load.

In relation to the difference of opinion between Mr. Johnson and Professor Lanza, as to the proper size of test specimens, the writer believes that each is right from his respective standpoint. Certainly, an investigator with the courage to attempt to formulate a plan of campaign to cover the territory outlined by Mr. Johnson would be unwise in the choice of large test pieces, with the additional variables involved in these pieces. On the other hand, Professor Lanza is right in insisting that the weaknesses incidental to large sticks of the species of timber found in his locality are not developed by tests on small sticks. A comparison of the relative qualities of the sap-wood of Hickory and Ash, for instance, must be predicated upon small test pieces. A comparison of the relative value of joists of Spruce and Long-Leaf Pine, on the other hand, must be predicated upon large sticks, of the sizes used in construction. With respect to the relation of tests to forestry, it is also true that the forester, in judging of the capacity of a stand of timber to produce saw-logs, must have in mind the actual market product, whether the large joists of Conifers, or the finishing wood of White Oak, or the carriage stock of the Oaks, Hickory and Ash.

The results of the former timber tests, criticized by Professor Lanza, were not discussed directly by the writer; but, since the matter has come up, he would say that these results have been used extensively by lumbermen, dealers, transportation companies, etc., in the same way that a large part of engineering data is commonly used, that is, as evidence to support opinion, as well as for a basis of design. Now, as indicating the relative essential properties of the fiber of different species, it is the opinion of the writer that the results of the former tests deserve the confidence that has been reposed in them by the public. These tests upon small sticks, which were made on a Riehlé testing machine, were not subject to the imperfections of the large machine in which the load was affected by the friction of a cup-leather packing. Since, as Professor Lanza points out, very few large

Mr. Hatt. sticks were tested, it must follow that the tabulated results of tests on small pieces represent results of tests conducted under proper conditions. Professor Lanza is probably right in his contention that these tabulated results are not applicable to large sticks. It is difficult to know just what was the quality of the test pieces sawed from logs, or what material is considered cull, what merchantable, what prime, etc. The writer would only remark that the average of results from later tests of about eighty-five large beams of Southern Pine and Red Fir agree with the average quoted in the publications of the former Division of Forestry.

Concerning the handling of the variable of moisture in large sticks, the plan adopted for the future, which involves testing these large sticks in a green condition, with a number of check tests on uniformly dry material, and on certain other material as found in the yards, is believed to be satisfactory. Tests on carriage stock may be performed on uniformly dry material. Mr. Johnson's proposal that the moisture present in the beam shall be determined by drying out successive horizontal layers of a disc taken from the beam would have merit were it not for the fact that the variation in these horizontal layers is nearly as great as it is throughout the entire section.

It ought to be plain that the tests outlined in the writer's discussion contemplate a somewhat broader field than, as is the view of Mr. Johnson, the breaking of a number of large market sticks. There is a golden mean, productive of useful conclusions, between the unintelligent collection of miscellaneous data, and the pursuit of a visionary programme, which would result in a mass of intertangled relations. As Mr. Johnson points out, there are more variables than equations. The writer firmly believes that this idea of a campaign to subdue all the problems in one investigation is entirely practicable for a material like concrete or reinforced concrete, and he could conceive of no more useful object of endeavor than a concerted effort by all the now isolated experimenters to put on a firm experimental basis the science of reinforced concrete construction But this elaboration of method cannot be carried over to timber physics or forestry. Does Mr. Johnson believe that a working plan can be formulated? The methods of precision, and the equipment of an astronomical expedition to observe the transit of Venus must be given up in the forest. Such a formidable and ponderous method of investigation as suggested by Mr. Johnson might be other than a merely interesting and attractive idea, were it not for the fact that foresters, who have most to do with matters of this kind, discourage the attempt. Nature has given a plain answer to most of these questions, and intelligent lumbermen and foresters have no trouble in understanding her language. What the testing machine can do in such cases is to supply quantitative results for relations which are very plain qualitatively.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

Paper No. 959.

LOADINGS FOR RAILROAD BRIDGES.

An Informal Discussion at the Annual Convention, June 11th, 1903.

SUBJECT FOR DISCUSSION:

"In view of the Increasing Weights of Rolling Stock, for what Loadings should Railroad Bridges be Designed?"

BY MESSRS. HENRY W. HODGE, J. W. SCHAUB, EMIL SWENSSON, THEODORE COOPER, A. J. HIMES AND HENRY W. HODGE.

Mr. Hodge.

HENRY W. HODGE, M. Am. Soc. C. E. (by letter).—The constantly increasing weights of rolling stock on railroads is a subject which affects most vitally the safety of all bridges on such roads; and, as few operating departments have made proper allowance for such increase in ordering these structures, it has been necessary to remove many good bridges and replace them with heavier ones within very short periods; generally much shorter than their natural life under loads for which they were designed. It is properly the field of the operating departments to keep the probable increase in loads in view when giving data to the construction departments; but though these live loads have been increased more than 100% in the last twenty years, with no certain signs of a maximum limit, it is seldom we find an operating manager who thinks there is the slightest necessity of designing bridges for greater loads than the latest locomotives he has ordered; and the writer has known railroads to order bridges on the same line for three different live loads within two years, so that the first bridges were overstrained 33⅓% within two years after they were completed.

Mr. Hodge. This appears to be a very short-sighted policy, and yet it is a question whether the bridge engineer is in a position to correct it, as his situation places him in the delicate position of appearing to wish to increase his remuneration if he suggests increasing the weight of a structure over what he is ordered to design. As stated previously, however, there is no sign that the maximum rolling loads have been reached, and if they are to continue increasing at anything like their past rate, it is certainly advisable for railway managements to give the subject more attention than it has heretofore received. It is evident to all engineers, and men in other departments of railroad construction and operation, that the tendency is constantly toward the moving of freight in larger units, and it is a well-established fact that the extremely low cost per ton of moving freight on American railroads has been reached by this use of larger units than are used elsewhere.

It seems to be agreed by all operating managers that increased economy would be further secured by enlarging the loads carried by each locomotive, and, therefore, it would appear that the final maximum weight of locomotive will only be found when the sustaining power of the roadbed is reached, or when it is impracticable to get more weight on the drivers of locomotives. We often hear the statement that locomotives cannot be made much heavier because the rails will not carry more, but this statement has been made continually for many years, and, all the time, the weights of locomotives have been steadily increasing and the weights of rails have been increasing about proportionately. The writer cannot see that the section of rails can ever be a bar to increasing live loads, as these rail sections can be increased to whatever point is desired, larger tie-plates being used, and it would appear that a much more serious difficulty to be overcome is the supporting power of the earth roadway and the rail joints. Ties cannot be spaced much, if any, closer than they are now, if bedded in the present manner in ballast, as the present spaces are about as small as possible to allow of proper tamping of ballast to hold the track in line and surface. If more bearing surface, with the present general arrangement, is desired, it will probably have to be gained by lengthening the ties, and there seems to be no good reason why this should not be done.

With the present arrangement of ties, bedded in good stone ballast, the pressure on the earth roadbed, from a driving axle weighing 60 000 lbs., spaced 5 ft. from center to center, can be assumed to be spread evenly over an area of 5 x 10 ft., or 50 sq. ft., giving a pressure of 1 200 lbs. per square foot. If this is doubled, for impact, there would still be only a little more than a ton to a square foot, which does not seem to be as much as a well-compacted earth roadbed should stand. Of course, this absolute pressure is not entirely the governing feature in the permanency of way, as it is necessary to have

the ties held in place so rigidly that they will not get out of line and surface. But it must be borne in mind that the maximum loads hauled will be heavy freight trains at a comparatively slow speed, and, in all probability, they do not have nearly as injurious an effect on the permanent way as the much lighter passenger loads moving at greater speeds. Thus it would appear that there is no reason why the maximum live loads should be determined by the supporting power, or the rigidity, of the permanent way. Mr. Hodge.

There still remains the question of what maximum weight it is mechanically possible to put over a driving axle, and the writer feels confident that this will be the governing point of the extreme axle loads. Locomotive builders have had their ingenuity taxed already to get the necessary weight on the driving axles to prevent slipping, and yet keep within the clearance height, and anyone looking at the modern heavy-grade locomotive is immediately struck with the fact that the top of the boiler is so near the upper clearance line that there is only room enough for the smallest apology for a stack and steam dome.

It must be remembered that the height and width of the locomotive is practically limited, and, as the present distances from center to center of drivers are made such that these wheels are as close together as possible, there seems to be no further space to get in more weight, unless the railroad clearances of height and width are changed, and this would mean practically rebuilding the railroads. It may yet be possible to decrease somewhat the size of the driving wheels and thus give more height above the axles for weights, but the modern heavy-grade engines have wheels less than 5 ft. in diameter, so that there is not much room for any considerable change in this direction.

As far as the writer has been able to get opinions from expert mechanical engineers engaged in the design of locomotives, it seems probable to them that the final maximum axle loads cannot practically be increased more than 15% above the present maximum axle loads, and the writer believes this to be a fair limit.

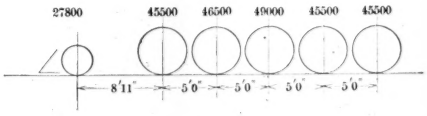


FIG. 1.

The heaviest locomotives for which the writer has seen any data are those built for the Atchison, Topeka and Santa Fé Railroad, weighing, without tender, 232 000 lbs. on five driving axles, and 27 800 lbs. on the truck, making 232 000 lbs. on a driving-wheel base of 20 ft., or 259 800 lbs. on a total wheel base of 28 ft. 11 ins., all as shown in the diagram, Fig. 1.

Mr. Hodge. Those built for the Pittsburg, Bessemer and Lake Erie Railroad weigh 225 200 lbs. on four driving axles, and 25 100 lbs. on the truck, making 225 200 lbs. on a driving-wheel base of 15 ft. 7 ins., or 250 300 lbs. on a total wheel base of 24 ft. 4 ins., all as shown in the diagram, Fig. 2.

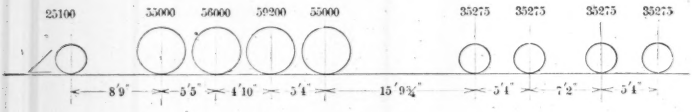


FIG. 2.

The latter engine is the heavier per linear foot, and the heaviest single driving axle carries 59 200 lbs., with an average for each driving axle of 56 300 lbs. If 15% be added to this average the result would be 64 745 lbs. per axle, and, from this, the writer is of the opinion that a locomotive having four driving-axle loads of 65 000 lbs. each will be sufficient to cover the extreme probabilities of rolling loads.

The tender of this Pittsburg, Bessemer and Lake Erie Railroad ocomotive has 35 275 lbs. on each of four axles, and increasing these by 15% gives 40 566 lbs.; but there are greater possibilities of increasng tender loads than there are for driving loads, therefore the writer would set the maximum tender wheel loads at 42 000 lbs. per axle.

If one forward truck axle is used, taking 32 000 lbs., the result will be an engine weighing 292 000 lbs., followed by a tender weighing 168 000 lbs., making a total of 460 000 lbs., complete.

It is hardly necessary in this general discussion to go into the detail of spacing of wheel centers for this theoretical maximum load, but the spacings now generally used in bridge specifications (which do not vary greatly) seem to be about correct, except the distance from the last engine driver to the first tender wheel, which is now generally 8 or 9 ft., and which probably should be increased to 14 or 15 ft.

The following uniform load still remains to be determined, and the heaviest loaded steel cars that the writer has been able to find are the iron ore cars, in use on the Algoma Central Railroad, which weigh 44 000 lbs., carrying a net load of 100 000 lbs., making a total load of 144 000 lbs. in an over-all length of 24 ft., thus giving 6 000 lbs. per linear foot, and if this again be increased by 15%, the result is 6 900 lbs., say 7 000 lbs. per linear foot. The writer, therefore, is of the opinion that it will be safe to use, as the probable maximum live loads for present railroad clearance dimensions, two locomotives, weighing 260 000 lbs. on drivers, and 460 000 lbs. complete, with tender, followed by a uniform load of 7 000 lbs. per linear foot. Of course, it is not necessary or advisable for all railroads to adopt any such maximum loads, as few roads will ever need such an extremely heavy class of rolling stock,

but this suggested maximum load is only intended to apply to such roads as have very heavy grades, and are looking forward to hauling eventually the greatest possible units over such grades. It certainly is not, and never will be, practical to adapt any particular set of live loads to the many railroad systems operating entirely different classes of traffic, under entirely different conditions. Mr. Hodge.

It is hoped that this suggested maximum load may lead to a full discussion of this important subject, and perhaps bring about more foresight in the ordering of new bridges, as such foresight will certainly be a benefit to the railroad corporations, even if it is at the expense of the bridge designers and constructors whose natural preference would be to keep replacing light bridges, rather than build but one in a life-time.

J. W. SCHAUB, M. Am. Soc. C. E.—The loading known as *E*-50, in Cooper's specifications, is now commonly used as a standard loading for railroad bridges. This provides for two engines weighing 355 000 lbs. each, including the tender, in working order. Bridges, if properly designed for this loading, will be able to carry with perfect safety a load fully 50% heavier than provided for. This means that the weight of engines can approach a total of 275 tons, before it would be necessary to even consider the renewal of the present bridges. When this loading has been reached, the present form of roadbed will long have disappeared. The speaker is not prepared to say what is to take its place, but if engineers can feel assured that steel bridges, as now designed, will carry any load that the present form of roadbed will be able to bring to them, that is all that should be expected of them. Between the roadbed and the bridges, it seems to the speaker that the bridges have the advantage just now, and he considers the *E*-50 loading to be more than ample to carry anything that can be carried on the present form of roadbed. Mr. Schaub.

E. SWENSSON, M. Am. Soc. C. E.—The speaker concurs in the last remark of Mr. Schaub, that it seems unnecessary for the present to consider any heavier loadings for railroad bridges than those of the heaviest engines mentioned by Mr. Hodge as being in actual service, and for which Mr. Cooper's typical loading, *E* 50, is about the equivalent; as this load seems to be the maximum that can be imposed upon the soil of some embankments and side-hill roadbeds. Mr. Swensson.

The above-mentioned heaviest engines go daily over bridges designed for somewhat less loading, without showing any effect on the bridges, but this is not always the case with the adjoining roadbed. It therefore indicates that, until some method of strengthening the roadbed is adopted, the engine-load should not be increased and consequently no increase should be made in the loading for railroad bridges.

THEODORE COOPER, M. Am. Soc. C. E. (by letter).—That the loads on freight cars, or the weights of engines, will continue to increase per Mr. Cooper.

Mr. Cooper. running foot of track to an unknown or indefinite extent is, in the writer's opinion, a most erroneous assumption.

Excepting special lines of road, carrying one kind of freight between definite terminal points, as certain mine, quarry or ore roads, the interchange of traffic between the numerous lines composing the railroad system of the American Continent demands that all cars shall be able to pass freely over each line, without surpassing vertically or laterally the limiting clearances, which determine the limiting cross-section of the train, including engines and cars.

While some roads have cars in use locally which would not be accepted on other roads, it is very certain that the great interchange of freight will ultimately compel the adoption of standard cars which can pass freely over all lines.

The American Railway Association has recently adopted a standard box-car,* which, presumably, is as large as would be acceptable to all roads.

It has the following inside dimensions: 8 ft. high, 8 ft. 6 ins. wide, and 36 ft. long.

The inside cross-section of this car, 68 sq. ft., when reduced to the length of track covered (distance between bumpers), gives about 65 sq. ft. as the maximum storage capacity per linear foot of track.

By reference to the writer's paper,† "Train Loadings for Railroad Bridges," in Table No. 3, it will be seen that this car could be completely filled with any kind of freight, excepting minerals and metals, without exceeding 4 000 lbs. of freight per linear foot of track. Practically, it would be impossible to fill this car completely.

If we allow (as would appear liberal) 1 000 lbs. per foot of track for the weight of the car, the maximum load per foot for fully 90% of the freight handled on our roads could not exceed 5 000 lbs.

Of the remaining 10% (excluding special ore-carrying lines) the larger proportion consisting of machinery, structural work, manufactured articles, etc., could not be stored within the limiting cross-section so as to exceed the above maximum.

Such cars, with this maximum loading, would weigh 190 000 lbs., which, with four axles, would give 47 500 lbs. per axle, or, with six axles, 31 667 lbs. per axle.

The self-dumping 100 000-lb. coal car has 36 500 lbs. per axle and 4 300 lbs. per foot of track.

As such loads on 33-in. wheels, when the wheels once become worn or roughened, would be far more trying to the track than the heavier driving-wheel loads, owing to the larger diameter and better care, it is certain that there must be a limit to such loads, as determined by longer experience.

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* *Railroad Gazette*, July 3d, 1903.
† *Transactions*, Am. Soc. C. E., Vol. XXXI, pp. 174 and 183.

Mr. Cooper.

Considering the small percentage of the freight which could be stored in the limiting cross-section so as to give greater loads per foot of track than 5 000 lbs., and the impossibility of keeping such cars in continuous circulation with full loads, except upon the specially excluded ore roads, their introduction upon the general railroad system is improbable.

It would seem to be justifiable, therefore, to assume that there is an upper limit to the freight load per running foot of track, and that 5 000 lbs. covers the limitation, unless it is assumed that the existing railroads will increase their present clearances, a tremendous undertaking in time and money.

Engines, also, must be limited in size and weight by the limiting cross-section of the roads, if not by the permissible axle loads.

With the limitation of the driving-wheel base, increased weight on the drivers demands more space for the greater boilers, cylinders, grate surface and running gear.

In *The Railroad Gazette* of October 27th, 1899, there is a theoretical examination as to the heaviest engine which could be built, under certain assumptions and with the usually accepted road clearances. Whether the conclusions of that writer are exact or not, it is interesting and instructive, as showing the influence of the allowed cross-section of a road upon the maximum possible engine, and indicates that, under present restrictions, we have about reached, in existing engines, the maximum possible. His maximum requires five coupled driving axles with a fixed wheel-base.

For further increase in the weight and power of the motor, we must either look forward to the possibility of electricity or to a series of combined motors with articulated wheel-bases.

It is reported that the Baltimore and Ohio Railroad has now under construction an engine with 285 000 lbs. on six driving axles with an articulated wheel-base, the three forward and three rear drivers each having an individual wheel-base of 10 ft. and a total wheel-base of 30 ft. 6 ins.

Such an engine, however, would not produce any greater stresses on bridges than the typical consolidation engines with the same load per axle.

It would appear to the writer, therefore, that maintenance-of-way conditions, weights on axles and car wheels, and existing clearances, all indicate that we are at or close to the maximum train loadings, under the present construction and limitations of our railroads.

On October 9th, 1899, the writer recommended the adoption of his train loading, *E*-50, by one of our important roads, as being, in his opinion, sufficient to cover all future load developments, when combined with the conservative unit strains now in general use; strains, which in the past have been very greatly exceeded, on nearly all roads,

Mr. Cooper. without risk or detriment, by the heretofore constantly increasing loads imposed.

It is not apparent, at present, why this opinion should be revised.

Mr. Himes. A. J. HIMES, M. Am. Soc. C. E. (by letter).—Since Mr. Hodge has been unable to find any cars in service having a greater load on the axles than those of the Algoma Central Railroad, it may be interesting to note that the Monongahela Connecting Railroad, of Pittsburg, is using cars, about the plant of the Jones and Laughlin Steel Company, which have a capacity of 200 000 lbs., and weigh 40 000 lbs. The length of the shortest of these cars is 33 ft. over all, making a total weight of about 7 300 lbs. per foot. The distance from center to center of axles is 5 ft. 6 ins., and from center to center of trucks is 23 ft. 3 ins.

Since these cars are in actual service, it is more than likely that the ore-carrying roads will soon find it expedient to put them into general use.

Past experience surely does not indicate that we have yet reached the maximum live load. To say that a thing will not occur, or that it is impossible, savors of omniscience, while to say that one certain thing is possible requires only a specific knowledge.

But while future increases of live load are probable, they will doubtless be of a special nature and not call for anything like universal adoption. A road on which the principal traffic is in grain will not be able to use rolling stock as heavy as that required by the ore-carrying roads, and in the same manner there is likely to be a greater differentiation of live loads in the future. Of course, many roads are occasionally obliged to refuse certain classes of freight because of great concentrated loads, and, as the country develops, such cases will become more common, but it is wholly impracticable for the smaller roads to equip themselves to handle the heavy loads that may form the bulk of the traffic on special roads.

A thorough appreciation of the possibilities of future increase of live loads would tend to increase largely the use of stone bridges.

While present economy may indicate the wisdom of replacing old bridges with new ones of heavier section, a strong faith in the country's continued prosperity would point to stone arches and earthen embankments as the best means of reconstruction.

Mr. Hodge. HENRY W. HODGE, M. Am. Soc. C. E. (by letter).—The writer has read with great interest the discussions on loadings of railroad bridges by Messrs. Schaub, Swensson, Cooper and Himes, and notes, from Mr. Himes' discussion, that the Monongahela Connecting Railroad is at present running cars weighing 7 300 lbs. per foot, which is evidence that the loads suggested by the writer are not in the least improbable.

Mr. Hodge.

The writer agrees fully with the statements made by all the discussors, that rolling loads for different railroads have to vary, and that it would be far from advantageous for many roads to design bridges for any such loads as the writer has suggested, as the loads proposed are only applicable to railroads that expect to carry the heaviest class of traffic.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

Paper No. 960.

IMPERVIOUS CONCRETE.

An Informal Discussion at the Annual Convention, June 11th, 1903.

SUBJECT FOR DISCUSSION:

"Is it possible to make Concrete which will be Impervious to Water? If so, what is the best method?

BY MESSRS. R. W. LESLEY, J. JAMES R. CROES, J. W. SCHAUB, B. R. GREEN, OSCAR LOWINSON, EDWARD CUNNINGHAM, W. K. HATT, THEODORE BELZNER, SANFORD E. THOMPSON AND WILLIAM B. FULLER.

Mr. Lesley.

R. W. LESLEY, Assoc. Am. Soc. C. E.—It is somewhat difficult to attack this subject, as the outlines of the field to be covered have not been very thoroughly defined.

Of course, it is known that there is a great difference between the porosity and permeability of mortar, though in a great many minds these two subjects are considered to be the same. It is known that, as an engineering question, the permeability of mortar is a serious one, and the porosity of mortar is only interesting from a scientific standpoint.

On the question of mortars and the subject of permeability, the most learned writer is M. R. Feret, Chief of the Boulogne Laboratory of the *Ponts et Chaussées*, a well-known expert of the French Government. In his work on this subject,* which has not been translated, Mr. Feret gives the results of a series of experiments begun by him in 1887, and

* "Sur la Capacité des Mortiers Hydrauliques," published in the *Annales des Ponts et Chaussées*, July, 1892.

pursued without interruption for a period of nearly five years. He gives the results of his experiments, embracing the most complicated tables and figures, covering in all some two hundred pages of press matter, and arrives at a number of conclusions which, in general, may be summed up as follows: Mr. Lesley.

1. The strength of mortars made on ordinary public works increases in proportion to the amount of lime or cement therein.
2. The strength increases generally at the beginning of the hardening in proportion to the size of the sand elements.
3. Mortars made with a mixture of sand containing large and small particles, present practically the same advantages as those where sands of large particles are used exclusively, and should be preferred to the latter.
4. The porosity of cement mortars varies very greatly. It diminishes as the proportion of cement increases. It is much greater when the sand is finer.
5. The permeability of mortars diminishes as the proportion of cement is increased; but, reversing the conditions applying to porosity, permeability increases according to the size of the sand grains. Mortars made with sand composed of different sized particles which themselves have little porosity, have also very slight permeability.

The permeability of mortars submitted to a continuous filtration of fresh water or sea-water diminishes rapidly with time.

It is recommended to mix mortars to a good consistency by adding too large rather than too small a quantity of water.

Coming more definitely to the question of the sub-heads of permeability and porosity, he states, under the head of porosity, the following as the conclusion of his experiments:

"That all mortars are porous when submerged in water, and the absorbed water will conform quite nearly to the total volume of the calculated voids; that, however, there is a very great distinction between the permeability of mortars, that is, the property they have of permitting percolation more or less by water, and their porosity, that is to say, the quantity of water they will absorb when immersed, the latter of which, as already stated, is about equal to the total volume of the voids. In general, the most porous mortars are the least permeable, and the reverse applies to permeable mortars

"The experiments showed that with coarse sand, the porosity diminishes, and it increases as the sand is fine."

To the question of permeability, Mr. Feret devotes many pages of his book, and has many experiments of a most interesting character. He finds that:

"Permeability is quite a different subject from porosity, and the effect of the passage of water may be said to free small particles of

Mr. Lesley. the lime or cement and bring them to the surface, where they produce efflorescence or fine stalactites, according to the freshness of the mortar. These efflorescences tend to solidify the mortar and form an exterior coating. Tables are given to illustrate by experiment the effect of this water passing through the permeable mass, which demonstrates that when water is forced through a mass of mortar, stonework or concrete, or is allowed to go through, the fact that large amounts do go through at once does not indicate any lack of value in the mortar, because the first hour may show a considerable quantity of water going through, carrying lime particles which close the pores, and as the process is kept up, each hour shows a diminished volume of percolation."

On page 97 of his book, in reference to the valuation of the permeability of the same mortar, there is a table showing the lessening of permeability, after a series of experiments, beginning at one hour and covering a week, at the end of which time there was practically no water coming through. This was due to the fact that the pores were obstructed by the carbonates of lime which result from the attack of the cement by the lime-carrying water passing through it, and its subsequent exposure to the atmosphere.

As a general rule, it is observed that this lessening of the permeability of the mortar is just so much more rapid as the filtration through the mass is more abundant. In reference to this it would appear that where, after a long drying, mortar is subjected to water and there is a great quantity pouring through immediately, this is the best indication that the mortar ultimately will be absolutely impermeable to water.

On page 99 of Feret's book, referring to this subject, a quotation is made, from a paper* by Durand-Claye, which confirms these experiments and lays down the principle that, for each class of mortar and sand, there is a definite dose of water which corresponds to the maximum of compactness and the minimum of permeability of the mortar.

In another series of tables, on page 100, with various proportions of water, it is shown that conditions govern largely the question of permeability, and also confirms the theory that the ratio of the permeability increases as the quantity of water increases. The permeability also depends upon the proportion of water, as stated above; and on page 101, there is a table showing that, at the end of a reasonable period, mortars made of varying proportions of water show about the same filtration, though at the beginning of the experiment there was a material difference in the quantity of water that percolated through them.

In another full series of tables, giving results with various sands, it is demonstrated clearly that the permeability is rapidly increased when the larger sizes of sand are used, while, on the other hand, decomposition is more active with mortars made with fine sand; and, in

* *Annales des Ponts et Chaussées*, Series 6, Vol. 15, p. 816, May, 1888.

summing up the subject of permeability, Mr. Feret comes to the following conclusions: Mr. Lesley.

"That in all mortars of granulometric composition, the most permeable are those which contain the least quantity of cement.

"Of all mortars of the same richness, but of varying granulometric composition, those which contain very few fine grains are much more permeable. They are the more so where, with equal proportions of the fine grains, the coarse grains predominate more in relation to the grains of medium size.

"The minimum permeability is found in mortars where the proportion of medium size grains is small, and the coarse and fine grains are about equal to each other."

In a very interesting paper by Messrs. G. W. Hyde and W. J. Smith,* a series of experiments to determine the permeability of cements and cement mortars was made, and these experiments were with both Portland and natural cements, neat and with varying proportions of water. They demonstrated very clearly that neat cements under pressures of 75, 100 and 200 lbs. per square inch, made in short 6-in. cylinders, 3 ins. in diameter, showed practically no permeability to water in 28 days, and only in the case of three of the samples in 7 days. With sand, however, the permeability increased as the percentages of sand were increased, the mixtures of 1:1 giving much better results than the 2:1 samples.

The conclusions which the writers drew from their experiments were:

"The results show that all cements are not permeable to water, at least for thicknesses of not less than 3 ins., while the mortars are all permeable; the amount increases with the pressure and decreases with age of specimen, but not in a direct ratio."

Comparisons are made by these writers between the permeability of cement and brick; and, referring to results on the Vanne Aqueduct, in France, they show that a conduit of béton-aggloméré, composed of sand and cement, showed practically complete impermeability.

Considerable attention was given to this subject by the Italian engineers in charge of the harbor works at Genoa, Italy, and many experiments were made to determine the porosity and permeability of mortars. Their conclusions were, that:

"The porosity of mortars depends upon the existence of voids of three kinds: first, those existing between the grains of sand or pozzuolana and not completely filled by the cement or lime; second, those due to the air which adheres to the surfaces of the grains; and third, those left by the evaporation of the water used in the mixing."

These three classes of voids were thoroughly investigated and discussed. The conclusions arrived at were: That cement mortars are less porous than those made of hydraulic lime and sand, or of ordinary lime and pozzuolana; that cement mortars made with coarse sand are less

* *Journal* of the Franklin Institute, Philadelphia, September, 1889.

Mr. Lesley. porous than those made with fine sand; that neat cement mortars are more porous than those made with sand; and that the proportions between the voids and the apparent volume of the mortar may vary from 12 to 46 per cent.

Paradoxical as it may seem, comfirming Feret's experience, however, the experiments showed that the permeability of mortars, so far from being a consequence of their porosity, is in fact almost inversely proportional to it. The exhaustive experiments made in this connection showed: That with the same proportions of cement, mortars made with fine sand are less permeable than those made with coarse sand; that, with the same quality of sand, permeability decreases as the proportion of cement increases; that mortar made with neat cement is the least permeable of all; that concrete made with 700 lbs. of Portland cement, 1 cu. yd. of mixed sand, and 1¼ cu. yds. of small gravel, and moulded in the shape of a hollow cylinder with a shell 2½ ins. thick; was impermeable under a head of water of 13 ft., while a mortar made with the same amount of cement and sand, but without gravel, and moulded in the same way, was somewhat permeable. Under a head of water of 27 ft., the concrete was barely permeable, while the mortar of cement and sand was very easily permeated. Concrete made with 1 150 lbs. of Portland cement, 1 cu. yd. of mixed sand, and 1¼ cu. yds. of small gravel, mixed very accurately with the quantity of water strictly necessary to work it up (say ⅓ cu. yd. of water), was impermeable under a head of 40 ft.

The effect of exposure to wave action, the labor required in the construction of different kinds of masonry, and the adhesion of mortar to different materials, were also studied carefully in these experiments.

The latest paper on the subject is one by Messrs. J. B. McIntyre and A. L. True, on "The Permeability of Concrete under High Water Pressures," which was presented before the Thayer School of Civil Engineering in 1902, covering experiments made in January to March, 1902. In many cases these experiments were made under conditions quite similar to those of Messrs. G. W. Hyde and W. J. Smith, the apparatus being of the same general character, and the pressures being 20, 40 and 80 lbs. per square inch, the time of the test lasting two hours. Two brands of Portland cement were used, and mortars of 1 : 1, 1 : 2 and 1 : 3 of sand were made. The conclusions drawn from the experiments are as follows:

"From the tables it will be seen that several of the mixtures were practically impermeable to water at the pressures which we had at our disposal, namely, from 20 to 80 lbs. per square inch.

"All the specimens composed of 1 : 1 mortar in the proportion of 30, 35, 40 and 45% of the whole mass were impermeable. Some of the specimens composed of 1 : 2 mortar in the proportions of 40 and 45% were also impermeable, as well as the mixtures of 1 : 2 : 4 and 1: 2½ : 4.

"All others leaked at the high pressure, and in a general way may be said to have shown a degree of imperviousness in direct proportion to the proportion of mortar in them, with the lower pressures, as well as with 80 lbs. However, as the object was to secure an impermeable product, the greatest attention was devoted to those mixtures which withstood the high pressure. Mr. Lesley.

"In the tests it has been assumed that the amount of evaporation was the same in all cases, as the time was the same in all the tests, and the temperature practically constant. But, at the same time, it must be granted that there will probably be some error, owing to the different consistency of the specimens and other unavoidable irregularities.

"On account of this, it is not well, in drawing conclusions, to lay much stress on the amount of water absorbed during the test as affecting the quality of the concrete in question; that is to say, when the amount of water absorbed was only a few grams and the specimen showed no sign of leaking. In fact, on leaving the specimens under pressure for 24 hours, it was found that in almost every case the specimen was lighter after the test than before, evidently owing to evaporation.

"The temperature of the room was about 50° Fahr., and remained nearly constant, and the air was far from dry, there being several tanks of water in the room continually.

"The results show a decided advantage for the specimens made from cement No. 1, both as regards the amount of water absorbed during the test and ability to withstand the highest pressure without sign of leakage. It appears also more plainly from the fact that cement No. 1 was procured in the open market, while cement No. 2 was sent by the manufacturers, who knew the object of its use. However, No. 2 was said to be more than two years old, and thus might not have been a first-rate sample.

"The specimens which showed no leak in 2 hours were left under pressure for 24 hours, and at the end of that time no leak appeared in any case.

"As a pressure of 80 lbs. per square inch is equivalent to a head of 184 ft., and the thickness of the concrete was only 5 ins., it is perfectly safe to conclude that a concrete of the proportions of any of the specimens which did not leak under 80 lbs. pressure would be practically impermeable under any conditions ordinarily found in practice.

"Of the various mixtures, we may safely choose either 1 : 2 : 4 or 1 : 2½ : 4, on account of their simplicity and the ease with which they may be proportioned, either for hand or machine mixing. In extreme cases, however, it might be advisable to use one of the richer mixtures."

As far as these general experiments on the question of the permeability of mortars show, they indicate conclusively that neat cement mortars show the least permeability, and that mortars with fine sand are less permeable than mortars with coarse sand, and that the lessening of the permeability is due to the closing of the pores by lime in suspension, which is carried through in the process of filtration through the mass, which ultimately forms a coating on the face of the masonry.

This state of facts was shown rather clearly in experiments by the late Alphonse Fteley, Past-President, Am. Soc. C. E., former Chief

Mr. Lesley. Engineer, New York Aqueduct. In order to make some of his masonry on the New York Aqueduct impermeable, he prepared a lime and cement wash, with which the surface of the masonry was painted, and which closed the exterior pores of the cement and made the work practically impermeable.

A practical illustration of Feret's experiments was shown in the case of a large concrete tunnel which was built at Hartford, Conn., where, after the construction, a large amount of efflorescence and small stalactites and stalagmites showed on the surface. Attention was called to this as a fault and defect in the masonry, but it was allowed to remain untouched, and the tunnel, though built in a particularly damp place, proved absolutely impermeable to water.

*The basis of all the various substances which it is suggested to add to mortars seems to be alum, fully as much if not more than soap. Alum has already been added in the making of hard plaster, and the material made is known as Parian cement, which is used in the manufacture of artificial marble and work of that kind, the addition of the alum forming aluminated sulphates of lime. Beds of natural Parian cement, as it might be called, have been discovered in Kansas, where silicated and aluminated sulphates of lime are found. These substances are water-proof, in which they differ from the ordinary plasters made of sulphate of lime alone.

It would seem that possibly a cement of that kind might be very finely ground and might be used for water-proofing impermeable mortars without introducing into mortars, soaps, alum or other substances, the chemical results of which are not thoroughly known in their bearing upon the duration of the mortar. It would certainly be a fatal thing to add an excess of alumina to the ordinary cement mortar where the use of concrete is to be in salt water. It would seem to the speaker that with these possible dangers, any change in the chemical constituents of the cement by the addition of new chemicals, the results of which are not known, is a subject that ought to be considered very carefully.

The fineness of the cement seems to be a very important feature in forming a coating to prevent permeability. It is known, from the examination of briquettes which have been kept in pans with water running through, that at early stages the briquettes when broken, even the neat ones, show considerable water through the entire mass; but, in the sand briquettes, the larger the admixture of sand, the larger the percentage of water. Now, when briquettes of neat cement are kept from month to month and from year to year, and are examined and broken, it is found that in the early stages there is a small core of dry material which forms in the center of the briquette—a small whitish-looking core which is absolutely dry—and as those neat briquettes are

* This part of Mr. Lesley's discussion was added after hearing the remarks of Messrs. Croes, Schaub, Green, Lowinson, Cunningham and Hatt.

kept for longer periods, and up to four, five or six years, it is found that that drying out or hardening from the center has continued and come out to the surface, while from the surface inward there seems to be hardly any percolation of water whatever. This is not the case with the sand briquettes. On the contrary, the core is formed much more slowly and takes much longer in coming to the surface. Thus, from this practical experiment it may be noticed—not under heads of water, but right under the ordinary flowing of the stream—that cement seems to take care of itself, as far as permeability is concerned, and largely in proportion to the fine grinding of the cement and the character of the coat that seems to be formed on the outside to repel the water; and the core that forms on the inside is largely dependent on the quality and the character of sand used. Mr. Lesley.

The speaker's theory and conclusions, based upon all that has been discovered, and especially upon Feret's remarkable experiments, would seem to indicate that the best addition to cement mortars for the purpose of making them impermeable, according to the theory of an exterior coating by lime carried through the mass during filtration, would be to add to the concrete, at the time of mixing, a reasonable proportion of hydrate of lime, or, in other words the ordinary slaked lime of commerce. This slaked lime, as has been shown by experiments of the late Professor De Smedt, formerly of the Laboratory of the District of Columbia, does not injure cements or mortars; does not cause expansion; and does not decrease their strength, though retarding their setting slightly. Such an addition would be perfectly safe, as a matter of practice, and would form in the mass a substance which would be carried by filtration and close the pores, form efflorescences or stalactites and stalagmites on the surface, and, in the speaker's judgment, though he knows of no experiments on the subject, would aid largely in making mortars impermeable. Experiments in this line would be most valuable.

J. James R. Croes, Past-President, Am. Soc. C. E.—The proposition for discussion is whether it is possible to make concrete which will be impervious to water. This is a very different thing from making an existing mass of concrete impenetrable by water. The latter requires only the application of an impenetrable coating to the surface of the mass. The former requires the entire interior composition of the mass to be such that water will not percolate through it at any point. Mr. Croes.

That a small mass of concrete can be made which will be entirely impervious to water, is incontrovertible. The question is, whether, in practice, a number of such small masses can be united in such a manner that the joints between them will be impermeable, also. Theoretically, there is no doubt that they can. Practically, the difficulty is in securing proper manipulation of the materials, at a reason-

Mr. Croes. able cost, and it is to be hoped that the discussion of the subject here will elicit descriptions of methods used by various practitioners in constructing large masses of concrete, and statements of the results obtained, both as to the efficacy of the means used and the expenditure required to produce certain results.

But the mechanical combinations and the manipulation of the materials required to reduce the void spaces in a mass of fragments of different materials to a minimum, and thus prevent the percolation of water, are not the only points to be considered. Chemical changes take place in materials thus compounded, and, of late years, studies have been made of the action produced by the introduction of other substances than stone, sand and water, into the composition of the mass which is to be made impervious, producing chemical action which reduces the porosity of the mass. It is to be hoped that anyone who has taken part in investigations of this kind will contribute to this discussion.

Mr. Cunningham's communication refers to an application of the Sylvester process, which was invented about sixty years ago and was used with success on the exterior of walls of buildings. It was applied to the gate-houses of the Croton Reservoir, in Central Park, New York City, and is described in the *Transactions** of this Society. That was a wash of soap and alum applied to the surface of a brick wall to prevent the percolation of water through that wall. In the case mentioned by Mr. Cunningham the experimenter mixed all his materials together, that is, the soap, the water and the alum were put in as constituent parts of the mortar itself, and the chemical action, which in the case of the wash occurred only on the exterior of the wall, was made applicable to the whole interior.

In the case of the cellar mentioned by Mr. Lowinson, an impervious material was placed between the layers of concrete. That was not making concrete impervious to water, but was simply keeping the water away from the concrete.

The question is, can the concrete itself be made impervious, and it seems to the speaker that the experiment mentioned by Mr. Cunningham is a most interesting one, and one which is capable of further amplification. That is, of producing the chemical change in the constituents, so that when they are all mixed and placed together, the water will not penetrate them. Chemical action takes place in the whole of the wall, and, somehow or other, the pores are filled up by the addition of the soap and alum.

Now, can we not make a concrete which contains some materials which will act together chemically? The composition of concrete is purely, or almost entirely, mechanical. Can some substance be introduced into the concrete by which a chemical change will take place,

* *Transactions*, Am. Soc. C. E., Vol. I, p. 203.

which will make that concrete thoroughly impervious to water? This experiment indicates that such can be done. Mr. Croes.

Although masonry is impermeable to water, a crack may occur in it, the same as in concrete. It is desired to make the concrete itself, in the mass, impermeable to water, but cracks cannot be prevented from occurring on account of unequal settlement, without very great care in construction. What is wanted is a material which, when made in a large mass, will be impermeable to water, provided it does not crack from unequal settlement, or expansion and contraction. Either wall or either material will be impermeable, but if a crack occurs, the water will go through it.

J. W. SCHAUB, M. Am. Soc. C. E.—In building an ordinary concrete cistern to hold water, all that is necessary to make the concrete impervious is to wash the walls inside with grout. Usually two coats of neat cement grout are sufficient for this purpose. This coating on the inside will hold the water in the cistern, but it is not impervious to water from the outside. In order to make a cistern impervious to water, inside and outside, it would be necessary to wash both sides, if possible, with grout. This is exactly what should be expected; the fine particles of cement closing up the pores of the concrete, similar to the caulking of the seams of a water-tank or boiler. In order to make a tank hold water it is necessary to caulk the seams from the inside; and, in order to make it impervious to water from the outside it is necessary to caulk the seams from the outside. The above applies to concrete when the water pressure does not exceed a head of, say, 10 ft. For pressures greater than that, the wash of grout does not suffice, and, in addition, the speaker has used asphalt, applied hot, with a mop, until a coat, $\frac{1}{4}$ in. thick, covers the grout, and the results have been entirely satisfactory. To what extent this can be carried, the speaker is not prepared to say, but he believes that a coating of asphalt $\frac{1}{4}$ in. thick, in addition to the grout, is sufficient for a water pressure of 60 ft., and he has recently specified this coating for this pressure. Mr. Schaub.

When the face of the wall to be water-proofed is accessible, it is a simple matter to apply the washes of grout and hot asphalt. If, however, the face is not accessible, the speaker knows of no method to be used to make that face impervious to water, excepting to build the wall in two parts, and fill the core or hollow space between the walls with asphalt. This method was used successfully in St. Louis, several years ago, in stopping leaks in a conduit, after every other method had failed. To be sure, a coating of asphalt is not permanent, but nothing is permanent in this world, and a coating of this kind, where exposed to the direct action of water, will have to be renewed about every ten years, the water acting on the asphalt as a solvent, as it does on everything else. Where the asphalt is used to fill up the cores in a wall, it will probably last as long as the wall.

Mr. Schaub. In case the concrete is reinforced with steel, it becomes almost necessary to make the concrete impervious to water, for it has been shown,* by M. Breuillé, a French engineer, that a chemical union takes place, between the metal and the cement, forming silicate of iron, which is soluble in water. When this salt has been dissolved, the bond between the metal and the concrete is destroyed, and therefore the combination of the concrete and steel is no longer steel-concrete, but a conglomerate of two substances which have nothing in common, excepting, perhaps, their coefficient of linear expansion. M. Breuillé has also shown that, where concrete has been protected by means of an asphalt covering, this union or bond between the concrete and the steel has not been destroyed by the action of the water. The conclusion to be drawn, is, that the concrete should have a coat of asphalt, or some other form of water-proofing, in order to make it impervious to water.

The speaker would suggest that this question, together with the entire matter of steel-concrete, be referred to a Special Committee of this Society.

Mr. Green. B. R. GREEN, M. Am. Soc. C. E.—The question appears to be simple. Of course, it stands to reason that any concrete to be water-tight must be so nearly solid—the interstices of the component materials or aggregates so well filled—that there shall exist in the mass no interstices or connected porosity, through which water can find its way from one side to the other. Most stones are permeable to water. It will soak into them. This is indicated by the tendency of some cements to stain stones that are embedded in or backed up with them, and by the earth stains found at greater or less depth in exposed rocks in nature. Building sand consists of very small stones. Even pure cement, after becoming indurated or set, will also soak water somewhat, especially if not ground to extreme fineness.

If, therefore, the materials of which concrete is composed are permeable to water, the concrete itself must be so, even if so well made as to be free from porosity or spaces between the stones, particles of sand, and cement. But, while water will soak through these materials, it is known that its progress is very slow, so slow that under slight heads of water thick and carefully built concrete walls will keep quite dry on the outside if exposed to drying air, the water evaporating as fast as it reaches the surface. This seems to be the whole story.

It would be very easy to determine by experiment the coefficient of soakage of pure cement for different thicknesses of wall and different heads of water, and the same for cement mortar with good quartz sand.

The speaker firmly believes that cement concrete can be made as

* *Annales des Ponts et Chaussées.*

water-tight as a stone wall, but not absolutely moisture-proof, without the application or insertion of a coating or core of some perfectly impervious material like asphalt. Mr. Green.

OSCAR LOWINSON, Assoc. M. Am. Soc. C. E.—About four years ago the speaker had an experience, recalled by the remarks of Mr. Green, in reference to the means by which concrete can be made water-tight. He tried to build a cellar on South Street, in New York City, which was to be made impervious to water against a head of about 6 ft. The building is located about 100 ft. from the dock line of the river. The intention was to put a damp course on a properly prepared bed of 6 ins. of concrete; this course to be water-proof, and on top of the damp-course 18 ins. of concrete, this having been computed to be of sufficient strength to resist the upward pressure of the water. The cellar was about 48 x 70 ft. in area. The theory was that if the damp-course were not punctured, the only work the upper layer of concrete would have to do would be to act as a medium to resist the hydraulic pressure; however, lest a leak might occur in the damp-course, this top layer of concrete was to be made most carefully, it having been the speaker's opinion that the concrete in it would keep the water out. The speaker was present during the greater part of the time when this concrete was being mixed. The job was done continuously, and, in that manner, no unfinished edge was permitted to set. Some six weeks after the completion of the job a sweat appeared at the lowest part of the cellar, which had been graded so as to permit baling out in case water might gather. At the end of another week several pailfuls of water had gathered, and, from that time on, the water found its way into the cellar at the rate of about a pailful a day. There was no question but that the water-proofing had been punctured, and also that the water had found its way over the concrete, but what surprised the speaker and caused him to conclude that his ideas as to the imperviousness of any concrete required revision was, that when he went to trace the source of the stream the puncture was found 18 ft. from where the dampness appeared upon the surface. Several attempts were made to repair this leak, but with no success, and the whole was finally removed, with the exception of the bottom layer, which had served as a bed for the water-proofing. Mr. Lowinson.

This cellar was made over twice again with the same result, costing the contractor more than five times the amount of his contract. The cellar was built finally in a different way. On top of the water-proof layer, a layer of brick in a bituminous mortar was placed, and 15 ins. of concrete placed on top of the brick. This was successful, and the cellar is perfectly tight to-day. The concrete used for the layer was composed of 1 part Saylor's Portland cement, 2 parts Cow Bay sand and 4 parts small broken stone. The speaker believes that the stone was not greater than 1 in. in diameter. Then there was a 2-in. cream of Portland cement and sand floated on, for a finished surface. The

Mr. Lowinson. concrete was mixed to a consistency that caused it to sweat when thoroughly rammed.

The concrete was mixed as follows: The sand and cement were first mixed and wet, the wet broken stone was put on top, the whole turned three times and the entire batch put in place within thirty minutes from the time mixing began.

Some of the remarks made in reference to this subject bring to the speaker's mind a paper which he heard read last spring, by Mr. Maximilian Toch, at a meeting of the New York Section of the Society of Chemical Industry, on the preservation of materials of construction, in which was propounded a theory which so interested the speaker that he suggested to Mr. Toch that a like paper read before the American Society of Civil Engineers would be productive of considerable discussion and most likely lead to valuable results.

The theory advanced by him was this: A thin layer of very fine cement, entirely free from all the elements that cause or aid oxidation or disintegration, can be applied so that it will change the initial condition of the surface exposed to dampness, so that, instead of being porous, or exerting capillary action, the effect upon the moisture or water would be similar to the action of oil on water, causing a repulsive, instead of attractive, influence. Mr. Toch was assisted in the preparation of this paper by Clifford Richardson, Assoc. Am. Soc. C. E., and the results that they had attained made the speaker believe that the theory was quite plausible.

The matter referred to interests the speaker in his building work, and he has used this material since as a cement wash on the surface of a building in New York City. It has now been in place some three months, and, so far, it appears to have kept out the moisture where, before that time, dampness had penetrated. It differs from the ordinary cement wash in that it sets almost immediately, does not flake, and is not washed off by the rain.

There is a possibility that concrete can be made impervious to water or dampness by an application of this kind, and this is a matter worthy of the best study that can be applied to it. In order to make it impervious, it would be necessary to apply this wash on both surfaces of the concrete.

Mr. Cunningham. EDWARD CUNNINGHAM, Esq. (by letter).—The writer recently made some tests for water-proofing mortar, which, though crude, were entirely satisfactory.

Using the materials at hand, two vessels, having an internal diameter of 11¼ ins. and an outer diameter of 12 ins., and about 26 ins. deep, were made in the following manner: A sheet of "chicken wire" was rolled and wrapped with wrapping paper. This was placed on a square board and filled with sand, making a heavy core. A split 12-in. terra cotta side-collector pipe was used for an outer shell and placed about the core, leaving a space of approximately ½ in. between the core and

the pipe. This space was then filled with rather wet mortar, which was worked down by "ramming" with thin narrow sticks. When filled to the top, a bottom was formed over the sand core. Mechanically, the two vessels were as nearly alike as they could be made from such simple materials. The paper was not a so-called water-proof paper. The pipes were slightly greased before placing, in order to prevent the cement from sticking. Mr. Cunningham.

The mortar placed in the first vessel was of the ordinary 2 : 1 mixture, in which was used Allegheny River sand, as it came from the pile. The mortar for the second vessel contained the same proportions of cement and sand, with the addition of 1% of the weight of the sand and cement in powdered alum, and 1% of the weight of the water in yellow soap. The alum was mixed in the sand and cement while dry, and the soap had been dissolved in the water.

The difference in the texture of the two vessels, when taken from the forms, was very noticeable. The water-proofed vessel was fine-grained and close, and was much smoother to the touch than the other. Lids of roofing slate were "rubbed" on after putting a ring of near cement around the top, in order to prevent evaporation.

After about three months the vessels were filled with water. The vessel made of the plain mixture immediately began to sweat, and "pin-holes" to weep. On damp days this was very noticeable. On dry, windy days the water on the surface of the vessel was apt to evaporate as quickly as it seeped through. The soap and alum vessel developed two or three "pin-holes," which showed slight weeping, otherwise it was dry. Without any records, the writer's recollection is that while the plain vessel was losing about 12 ins. the other vessel lost about 1 in., and that was through the "pin-holes," the surface never appearing generally damp.

In plastering the inside of a covered clear-water well, a potash soft-soap was used. The water for the plaster was used from a barrel into which 1¼ lbs. of soap was put for each five buckets of water. The alum was used from paper bags by the mixers, a bag of alum to a bag of cement. As the walls leaked badly the contractor did this part of the work conscientiously. The mortar was fine to handle with the trowel, but the masons, frequently nauseated by the odor, had to come out to ventilate themselves.

Two plastering coats were applied, both being not more than ½ in. thick. The writer's instructions were simply to cover up the stone and make no attempt to bring it to a surface. There was seldom more than ¼ in. in the coating.

The results were entirely satisfactory. The application was made when there was no leakage; and, when rains set in, in the fall, the walls remained almost entirely dry.

In the filtered-water regulating houses the 18-in. dividing wall showed no leak when one side held 16 ft. of water. In the clear-water

Mr. Cunningham. well there was slight leakage at the joint between the work from the floor, and that from the scaffold.

There are some soaps in which the potash is much stronger than in others. The "old-fashioned" soft-soap, made on the farms, from soap fats and wood ashes is one kind of potash soap, and has a dark color because clean fats are not used. The soap used by the writer was white, because made of clarified fats, and was expensive—7½ cents a pound—but should be obtained for much less. It came in bulk, in barrels.

Using 1 part cement to 2 parts sand, it takes from two to three 12-quart buckets of water for each barrel of cement to mix mortar to troweling temper. Concrete requires from six to eight buckets of water. The wetness or dryness of the sand (except the very extremes) may be disregarded, as laborers will vary the quantity of water, from batch to batch.

Lehigh cement is put up in bags of 96 lbs. each. When mortar is mixed in the proportions of 2:1 there will be approximately 100 lbs. of cement to 200 lbs. of sand, therefore the alum was put up in 3-lb. bags, and one bag of alum was used with each bag of cement.

Therefore it takes 3 × 4, or 12 lbs. of alum to each barrel of cement in 1 : 2 mixtures.

The extra cost would be:

Soap, 2 lbs. (with 8 buckets, or 200 lbs. of water) at 7½ cents	15 cents.
Alum, 16 lbs. (with 1:3 mixtures) at 3½ cents	56 "
Extra cost per batch of concrete	71 "

In a concrete proportioned 1:3:5, each batch will make about 0.75 cu. yd. of masonry in place. At that rate, the extra cost would be, approximately, $1 per yard. But both soap and alum, in larger quantities and in grades not as fine, should be obtained for much less money.

The writer, however, does not believe that it is at all necessary to build a concrete wall in this manner, as a 2-in. mortar face should be ample. In the filtration works at Apollo, a skin averaging less than ½ in. in thickness seemed to be sufficient.

Mr. Hatt. W. K. HATT, Assoc. M. Am. Soc. C. E.—Various solutions have been used to render concretes water-tight, or to protect stonework. Some of these solutions form the basis of patents granted for the manufacture of artificial stone. The speaker has experimented with two of these: silicate of soda, and alum and soap. The silicate of soda, called water-glass, is used locally in Northern Indiana as a surface coating for reservoirs and water cisterns. The soap and alum solution has been used for some time, as alternate coatings for the surface of masonry, under the name of Sylvester's wash.

Mr. Hatt.

Alum forms the hardening agent of many patent mixtures for making artificial stone. One patent, granted in 1876 to L. L. Leathers, for artificial stone, * prescribes a mixture of cement and sand moistened with a solution formed of a mixture of fat and lye with alum.

The speaker was confronted a few years ago with the problem of designing a mortar for use in forming the segments or plates out of which burial vaults are moulded. These burial vaults must be water-tight, and since the expense of freight soon limits the area to which shipments may profitably be made, the mortar should be as light as possible. The conditions were met by a design of a mortar of 2 cement, and 5 fine bituminous ash made into plates strengthened by ordinary poultry mesh. Experiments were conducted with silicate of soda, and with the alum and soap solutions, to determine the effect of these on the strength and porosity of the mortar. The following results, in general, are of interest.

The effect of silicate of soda is to diminish the strength of both ash and sand mortars more than 50%, and to diminish the absorption of the ash mortars about 50 per cent.

The effect of alum and soap, mixed in with the mortar at the time it is gauged, is to strengthen and harden the ash mortar about 50% and to diminish its absorption by 50 per cent. A soap solution alone will diminish the absorption (by the action of the alkali in the cement on the soap) but will not increase the strength.

The strength of the sand mortar is not greatly affected by the soap and alum, but its absorption is decreased about 50 per cent. The effect on the absorption was measured by comparison of the weight of water taken up by briquettes which were immersed after having been dried out. Check tests were made by measuring the water which percolated from the outside through the walls of hollow cylinders.

The speaker believes that this is the first use of a soap and alum solution for water-proofing, in place of the usual gauging water.

The method used by the speaker is as follows: A 5% solution of ground alum and water is prepared; and a 7% solution of soap and water. The alum solution is mixed with the mortar to the amount of one-half the ordinary gauging water. The soap solution is then applied in amount to bring the mortar to the desired plasticity. The soap and alum, acting together, cause the precipitation of an insoluble compound in the pores of the mortar.

A solution of lye and alum is said to produce the same effect.

In regard to the problem of making pipes, the French engineers believe that in order to make a concrete pipe hold water, a sheet-iron tube must be placed inside of it when the head is about 50 ft., and such pipes have been used under a head of 300 ft. When the head is less than 50 ft. they make a reinforced concrete pipe without any steel lining.

* Letters Patent No. 178 307.

Mr. Belzner. THEODORE BELZNER, Jun. Am. Soc. C. E. (by letter).—The writer has read with much interest the discussions on "Impervious Concrete," and recalls having seen specifications of water-proofing calling for two layers of felt in ground that is quite dry, and where there is water pressure against the masonry equal to 12 ft. there should be used not less than six layers of felt. When the water pressure is less than 12 ft., or where the ground is damp, from three to six layers should be used, such being left to the discretion of the engineer in charge.

The writer firmly believes that concrete can be made impervious to water, but not at a reasonable cost, on account of the expense of the proper application of the materials.

Some time ago the writer examined a jack-arch where moisture appeared, and after cutting out the concrete, about 18 ins. square and 12 ins. thick, the water-proofing behind it was found to be punctured and had bulged about 8 ins., caused by the pressure of the surface water. The water-proof paper was removed and replaced, and the section of the arch replaced with brick dipped in hot asphalt, and since then no leaks have occurred.

The writer has had some experience in locating leaks in steel-concrete construction, and in jack- and roof- arches, and has traced to their source leaks caused by surface water, and in some cases, has found the puncture to be from 5 to 20 ft., or more, from where the dampness appeared upon the surface.

When moisture appears on a wall it does not necessarily indicate that the leak is directly behind it; the source may be a great distance from where the sweat appears.

Mr. Thompson. SANFORD E. THOMPSON, Assoc. M. Am. Soc. C. E. (by letter).—The writer's attention was first called to the possibility of making pure concrete of very thin section impervious to water, by a visit to the filtration works of the East Jersey Water Company, at Little Falls, N. J.,* through the courtesy of the Resident Engineer, William B. Fuller, M. Am. Soc. C. E. A circular wall of reinforced concrete, 15 ins. in thickness at the bottom, and withstanding, either upon the inside or the outside, a pressure of water of more than 40 ft. in depth without leakage, proves beyond question the possibility of laying concrete which is practically impermeable. Another example of concrete construction, at the same plant, no less remarkable, in view of the difficulties usually encountered in placing a thin wall of reinforced concrete, is a circular tank, 4 ft. in diameter and holding water to a depth of 4 ft., with wall and bottom only 3 ins. thick.

The double-track tunnel between Boston and East Boston, built under the harbor by the Boston Transit Commission†, Howard A.

* See paper describing this plant, by George W. Fuller, Assoc. M. Am. Soc. C. E., *Transactions*, Am. Soc. C. E., Vol. L., p. 394; and discussion thereon by Mr. William B Fuller, on p. 453.

† See Seventh Annual Report, Boston Transit Commission, 1901.

Carson, M. Am. Soc. C. E., Chief Engineer, illustrates the construction, under difficult conditions, of concrete which could not be laid as a monolith, but had to be built in sections 30 ins. in length. The thickness of the arch and walls is 33 ins., and at high water the depth of the roof below the surface of the harbor may be as great as 70 ft. The writer observed, when walking through the tunnel soon after the air pressure was removed, that the only leakage consisted of percolations at the joints between the sections of concrete laid on different days. These emitted a scarcely appreciable volume of water, and were being closed by forcing in neat-cement grout under pressure. Mr. Thompson.

It is generally agreed by engineers, in this country and abroad, who are accustomed to placing concrete which is to withstand the percolation of water, that the essential elements for impermeability are:

An aggregate (including sand) proportioned for great density;
An excess of cement;
A wet mixture carefully placed;
Monolithic construction.

The addition of foreign substances, such as soap and alum, slaked lime, or pozzuolana, has for its chief object the introduction of very finely divided matter into the pores of the concrete, and as long as the cohesion of the cement is not injured, it makes very little difference, as regards the permeability, whether their action is chemical or mechanical. A mechanical addition may actually increase the strength as well as the impermeability, simply by increasing the density, and, therefore, may replace a portion of the cement, which, as the finest ground material in ordinary concrete, must otherwise fill all the residual voids in the aggregate, with a surplus to provide for imperfect mixing. Clay has been suggested as a suitable ingredient for increasing the density, as laboratory tests tend to show that a small admixture of clay increases rather than diminishes the strength of mortars. While in the laboratory this might be proved effective, its use in the field would probably be impracticable because of the tendency of its particles to adhere, and form balls having slight cohesion.

M. Feret, in France, has recently been conducting a series of experiments* to determine the effects of mixing, by grinding together, materials of a pozzuolanic character with cement, to be used in sea water. Specimens in the air were injured by the introduction of this material, but those exposed to sea water were improved. By comparison with specimens to which had been added matter of the same fineness as the pozzuolana, but inert in character, the action is proved to be, at least in part, a chemical one, and the results not merely due to the greater density obtained by the more perfect filling of the voids.

The relation between porosity and permeability, which has been

* *Annales des Ponts et Chaussées*, 1901, Part IV, p. 195.

Mr. Thompson. referred to by Mr. Lesley, is misleading. Although M. Alexandre, as quoted by M. Feret, shows that the more porous mortars are sometimes the less permeable, this principle cannot be applied strictly to concrete, because, in a porous concrete, as the term is generally accepted, the voids are apt to be of such size that the capillary action of the water contained in them is insignificant. One of M. Feret's most important deductions has a clear bearing upon this point:

"It must not be understood, however, that the quantity of water which, under a given pressure, traverses a mortar during a given time is necessarily proportional to the total volume of voids in the mortar. It depends especially upon the size of the voids considered separately. Thus it is that if one compares a series of mortars obtained by combining equal weights of the same cement with sands of increasingly large dimensions, it is noted that the permeability may be very slight for the first and considerable for the following, although the real volume of solid material entering into the unit volume of the mortars goes on increasing at the same time with the size of the sand."*

Mortars made with fine sand, however, M. Feret states, although less permeable, are apt to be more easily acted upon by sea water. Considering, now, mixed sands, M. Alexander has shown by experiments that these are less permeable than either coarse or fine sifted sand alone, and Table No. 1, from his paper on Hydraulic Mortars,† shows the results of his experiments with different sizes of screened sand, and also the effect of time, and of varying the proportion of cement to sand, upon the percolation of water through a thickness of 4 cm. of mortar.

TABLE No. 1.

Character of Mortar.				Time for 0.025 Liter of Water to Percolate under a Head of 0.50 M.		
Size of sand.		Quantity of cement per cubic meter of sand.	Proportions of cement to sand by weight.‡	At the beginning.	After one month.	After two months.
Passing screens with meshes per decimeter:‡	Corresponding to screens having meshes per linear inch:‡					
Coarse (25 to 45).	6½ to 11½	250 kgr.	1 : 5.4	Instantaneous.	Instantaneous.	1 minute.
		400 "	1 : 3.4	Instantaneous.	15 minutes.	24 hours.
		550 "	1 : 2.4	5 seconds.	24 hours.	.....
Fine (175 to 280).	45 to 72	250 "	1 : 5.4	45 seconds.	2 hours.	.....
		400 "	1 : 3.4	20 minutes.	5 hours.	.....
		550 "	1 : 2.4	45 minutes.	24 hours.	.....
Mixed equal parts (25 to 45) (45 to 60) (90 to 110) (140 to 175) (175 to 290).	Equal parts (6½ to 11½) (11½ to 15½) (23 to 28) (36 to 45) (45 to 72)	250 "	1 : 5.9	1 minute.	3 hours.	.....
		400 "	1 : 3.7	45 minutes.	30 hours.	.....
		550 "	1 : 2.7	3 hours.	Several days.	.....

* *Annales des Ponts et Chaussées*, 1892, Part II, p. 91.
† *Annales des Ponts et Chaussées*, 1890, Part II, p. 407.
‡ Columns compiled by the writer from data given by Alexandre.

Mr. Thompson.

The mixed sands undoubtedly reduce the size of the voids, and as at the same time they give greater density, they are consequently to be selected in preference to either the coarse or the fine. Extending this principle to the coarser materials of the aggregate in concrete, the conclusion is reached that the selection of materials to produce greatest impermeability coincides with the selection of materials for maximum strength, that is, the proportioning of the aggregate so that the grains of any one size will be sufficient in quantity to fill the voids of the larger sizes.

It is obvious that a mass containing voids or "pockets" such as we invariably see in concrete mixed with an exceedingly small percentage of water, and sometimes also in concrete mixed wet but laid carelessly so that the mortar is allowed to run away from the stones, is more permeable than a mixture which to the eye is dense and compact. In regard to the more minute voids the conclusion is not so clear, the advocates of dry-mixed concrete claiming that the surplus water when evaporating leaves voids which reduce the density of the mass. Mr. William B. Fuller, a champion of the wet mixture, has, on the other hand, asserted that, in his belief, the greater specific gravity of the solids causes them to expel the surplus water. Experiments, in collaboration with Frederick W. Taylor, M. E., which the writer now has under way, and which he hopes at a later date to present to the Society, tend to prove the correctness of Mr. Fuller's theory. Careful measurements of specimens, of both mortar and concrete, made with varying percentages of water, show, after setting, almost identical volumes, and, therefore, nearly equal density. A dry mixture, however, to bring it to this point, requires very violent ramming, harder than can be given in practical work, while the very wet mixtures need to be merely "joggled."

With the latter specimens, water, varying in amount with the quantity used in the mix, rises at once to the surface, and if the mould is tight and the solid materials are carefully proportioned, it may remain there until it evaporates, or until taken up by the capillary action of the mass.

Mr. Fuller.

WILLIAM B. FULLER, M. Am. Soc. C. E. (by letter).—This topic is immediately divided, in the writer's mind, into two:

First.—Is it possible to make concrete so that the concrete mass itself is impervious to water?

Second.—Is it possible to design concrete structures so that, no matter how they may be affected by temperature and other external causes, they will still remain water-tight?

Taking up this question from the first point of view, it would seem that it is purely a question of finance. It is well known that neat cement is practically water-tight under ordinary pressures when made into hardened mortar, well trowelled down on all surfaces which come

Mr. Fuller, into contact with water, and is as impermeable as the best of natural rocks used for building purposes. The addition of sand and stone to cement, while, incidentally, it has other advantages, is done principally to reduce the cost, and the question as to the extent to which this can be done without reducing the impermeability of the mass is, the writer assumes, the extent of the topic under discussion.

It has been generally assumed by many writers on the subject, in textbooks and elsewhere, that the addition of as much as 2 parts of sand and 4 parts of stone will have no practicable effect in increasing the permeability of the mass. It seems to the writer, however, that this question of permeability is one intimately related with the skill in which the ingredients of the concrete are mixed together and placed in position.

It is well known that by unskilful methods of manipulation even the very best materials can be so placed in position that there will be channels established throughout the mass through which water can percolate, and it is also conceivable that by an ideal mixture of simply properly graded sizes of sand and stone, impermeability to water could be obtained without the use of any cement whatever. Somewhere between these two extremes will lie the mean of ordinary practice in concrete construction.

Stones having a diameter of, say, 3 ins. when placed together will have large voids between the adjacent stones. If now these voids are filled with other stones of exactly the right size and the voids thus obtained are again filled with smaller stones and this grading is continued down until very fine sand is being used, so fine that capillary action will prevent the passage of water under ordinary pressure, the result will be an impermeable mass.

In an ideal mixture, therefore, cement would be used only as representing the very finest of sand in filling the capillary voids, which were left by other materials, and for holding these materials in their proper places.

In practice, it has been assumed, ordinarily, that the cheapest of labor is sufficient for the making of good concrete, it being customary to use large quantities of cement and pay little or no attention to the reduction of voids to their smallest dimensions by the grading of the stone and sand. As a matter of fact, the use of unskilled labor, no matter what the richness of the concrete may be, always introduces the probability that a portion of the mass will be placed improperly and may therefore be permeable; moreover, even if the richer concrete could be made water-tight by unskilled labor, it still would be cheaper to employ skilled labor, as the skilful mixture of the ingredients will allow of a large decrease in the quantity of cement, and result in a uniform and impermeable product.

For example: With care in the selection, mixing and placing of the ingredients, concrete having the proportions of, say, 1 part cement,

3 parts sand and 7 parts stone should be as impermeable a concrete having the proportions of 1 part cement, 2 parts sand and 4 parts stone. In one case there would be about 1½ bbls. of cement used in each cubic yard of concrete, and in the other case there would be about 1 barrel. This represents a saving of 33% in the value of the cement, while the additional cost of the skilled labor would be very small. Mr. Fuller

The question as to whether concrete should be placed by the wet or by the dry method is one of secondary importance, provided the proper skill is obtained. The general trend of modern concrete construction is to place the concrete very wet, thus insuring the filling of voids with less skill on the part of the laborer than is required when the concrete is placed dry. The writer is of the opinion that considerable extra insurance as to the quality of the work can be secured by reversing the ordinary process of machine mixing. In order to secure impermeability, it is absolutely necessary that every particle of sand be coated with cement, and that this cement and sand surround every particle of stone, so that the stones or the sand grains do not actually come into contact with one another. To insure this result, the writer is a strong advocate of machine-mixed concrete, using one of the newer types of machines, such as the Ransome, Smith or Chicago mixers, into which the water can be gauged accurately, and in which the entire process of mixing is continuously under the eye of a skilled observer. By the use of such a machine, introducing a measured quantity of water first, and then introducing the cement, there is made a liquid grout which will run easily into the most minute voids of the sand. The sand being introduced next into this grout becomes coated in the shortest space of time, and the mortar thus made is still quite liquid and enabled to flow into all the voids of the stone. In the writer's opinion, a much more intimate contact between the materials of the aggregate is secured thereby.

Taking up the second part of the discussion, the writer believes the question can only be answered by experience. Small structures, say not exceeding 60 ft. in any dimension, can be designed so as to take up temperature stress without the introduction of iron, but very careful attention must be paid to all details; the fundamental principle being that all parts shall be equally strong, so that the whole structure acts as a monolith, expanding and contracting from one point as a center. By the introduction of iron, cracks can be confined to certain definite places which may be treated as joints, and, in the majority of cases, such joints may be made water-tight. The ideal water-tight joint, however, has yet to be invented, and it is in this direction that we must look for improvement in the design of large water-tight concrete structures.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

Paper No. 961.

AUTOMATIC MODULES FOR REGULATING THE SPEED OF FILTRATION.*

By Charles Anthony, Jr., M. Am. Soc. C. E.

With Discussion by Messrs. John H. Gregory, W. R. Copeland, J. W. Hill and Charles Anthony, Jr.

Now that the importance of keeping the velocity of sand filtration constant, and below a given maximum, while providing the means of gradually increasing that velocity after each cleansing of the sand surface, has come to be universally recognized, it has occurred to the writer that a short paper treating on the various forms of module, or controller, that have been designed with this object might be of interest.

Numerous forms of semi-automatic apparatus of this description have been devised, but as, under the law of the survival of the fittest, they appear to be doomed to speedy extinction, at all events for the purpose in view, it has not been considered expedient to include any special mention of them herein.

Before proceeding further, it may be well to point out the requirements which an apparatus of this nature should fulfil. They are twofold:

1st.—It should be susceptible of adjustment to any constant discharge between the limits zero and the maximum volume required.

* Presented at the Meeting of April 1st, 1903.

2d.—It should be mechanically simple, and of a durable nature, not requiring skilled attention.

All modules that have been designed by, or have come to the notice of, the writer, of which a list is appended in Table No. 1, belong to one or other of the following four categories:

1st.—A weir or orifice, whether surface or submerged, maintained under a constant head by its own movement.

2d.—A fixed, submerged weir, or orifice, kept under constant relative head.

3d.—A governed motor, passing a constant volume at a constant velocity.

4th.—A submerged orifice, closed automatically in such ratio to the head as to give a uniform discharge.

TABLE No. 1.

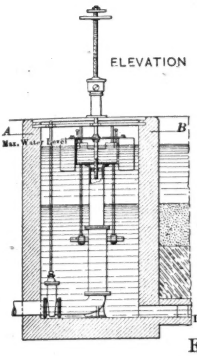
Description of Module.	Means of Varying the Discharge.
First Category.	
1. Surface weir supported by float at constant depth	Submergence variable.
2. Surface weir, as above	Width of weir variable.
3. Submerged weir, as above	Submergence variable.
4. Submerged weir, as above	Size of orifice variable.
5. Floating siphon	Head variable.
6. Floating siphon	Size of orifice variable.
Second Category.	
7. Balanced valve, keeping pressure constant on orifice	Pressure variable.
8. Balanced valve, keeping pressure constant on orifice	Size of orifice variable.
9. Head on submerged weir kept constant by relative motion of floats on up- and down-stream sides	Head variable.
10. Head on submerged weir kept constant by relative motion of floats on up- and down-stream sides	Size of orifice variable.

Description of Module.	Means of Varying the Discharge.
THIRD CATEGORY.	
11. Turbine or motor, driven by passing water, and governed.	Speed variable.
12. Turbine or motor, driven by passing water, and governed.	Capacity of motor variable.
FOURTH CATEGORY.	
13. Paraboloid moved in orifice by float	Discharge cannot be varied.
14. Disc or sphere moved in paraboloidal tube by float.....	Discharge cannot be varied.
15. Pendulous obstruction moved and varied by velocity of passing water.....	Discharge cannot be varied.
16. Flat, cylindrical or other shaped valve with uniform motion derived from float, but with curved orifice or orifices.....	Variable, in the latter case by closing off one or more orifices.
17. Rectangular aperture or apertures, in flat plate or cylindrical valve, susceptible of two rectilinear, or rotary and vertical movements, one differential and automatic; the other adjustable, to vary discharge.	

The most self-evident forms of apparatus for attaining the end required belong to the first class. The Glenfield Jones, a simple surface-weir variety, is illustrated in Fig. 1, and it will be seen that the amount of discharge is regulated by varying the depth of submergence of the weir. This, however, is an imperfect type, as friction causes the weir to be more or less submerged, depending on whether it is floating on rising or falling water, and, furthermore, the adjustment for varying the discharge is cumbersome and inconvenient.

A better arrangement is that which is illustrated in Fig. 2, in which it will be seen that the substitution of a submerged form of weir reduces greatly the error due to friction, since difference of submersion has less effect. The adjustment of the discharge by the closure

AUTOMATIC MODULES
FOR REGULATING THE SPEED OF FILTRATION.



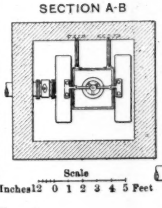
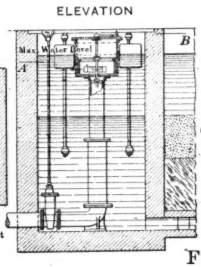


FIG. 1



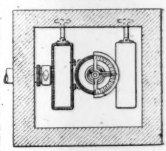
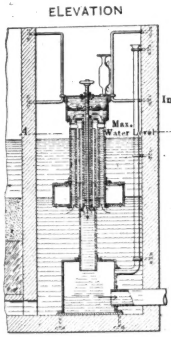


FIG. 2



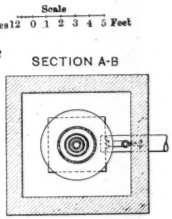


FIG. 3

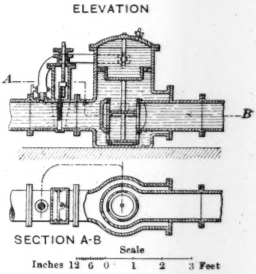


FIG. 4

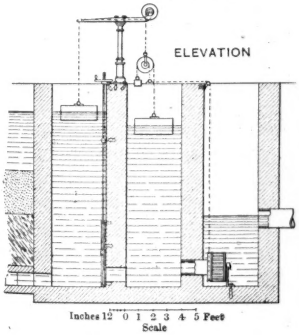


FIG. 5

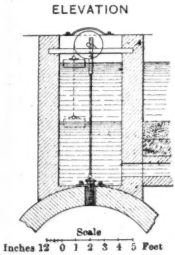


FIG. 6

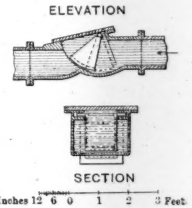


FIG. 7

of orifices is not only simpler, but lends itself to an easy method of indicating, on a graduated dial, the rate of discharge, seeing that the discharge is directly proportional to the amount of angular movement of the inner cylinder. This end may also be attained, with less friction, by the pneumatic device illustrated in Fig. 3; but the writer cannot say that this appears to him to be a hopeful form, on account of the difficulty of keeping the upper part of the siphon free from air.

To the Second Category belongs the constant-flow valve of Professor Fleming Jenkins, invented in 1876, of which Fig. 4 is an illustration.

A simpler form has been illustrated and described* by Professor W. R. Burton.

A difficulty encountered when trying a valve of this form, in Buenos Ayres some years ago, was the blithering set up, due to the constant oscillation of the valve about its mean position; but this could doubtless be overcome by a judicious damping arrangement.

Fig. 5 illustrates a somewhat different type of apparatus of the same class, which occurred to the writer on reading of an ingenious form of submerged weir-gauge constructed by Mr. John H. Tudsbery-Turner at the Yokohama Water-Works.

Class 3 needs no description, nor can the writer call to mind a single case in which this principle has been applied in practice.

A typical example of Class 4 is Juan Ribera's Spanish module, a form of which, designed by the late Mr. J. F. La Trobe Bateman for the City of Buenos Ayres Water-Works, is illustrated in Fig. 6. This would constitute an elegant solution of the problem, did the apparatus fulfil the first condition laid down. Unfortunately, it does not, as it is not susceptible of adjustment for varying the discharge for which it was designed; the prevalent idea that the discharge may be varied, without affecting the uniformity of the flow, by an initial alteration in the position of the cone in the diaphragm, being a delusion.

Fig. 7 illustrates Appold's module, the action of which depends on the dynamic effect due to variation of velocity. It is, therefore, necessarily inaccurate; and, furthermore, is not susceptible of adjustment for varying the discharge.

What the writer believes to be an original form of module occurred to him in 1893. The fundamental principle of this form consists in

* *Minutes of Proceedings*, Inst. C. E., Vol. CXII, p. 323.

AUTOMATIC MODULES
WITH ADJUSTABLE DISCHARGE.

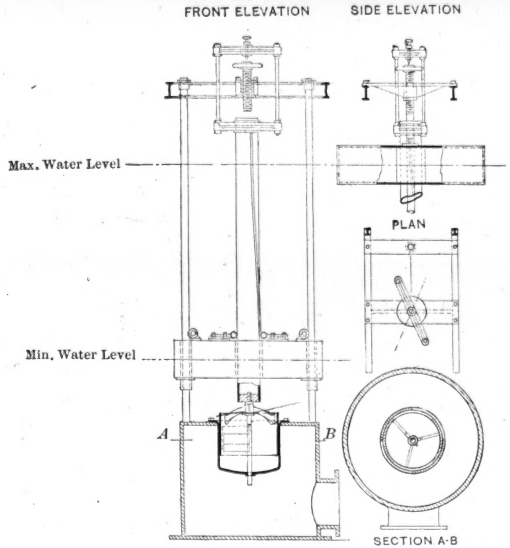


FIG. 8

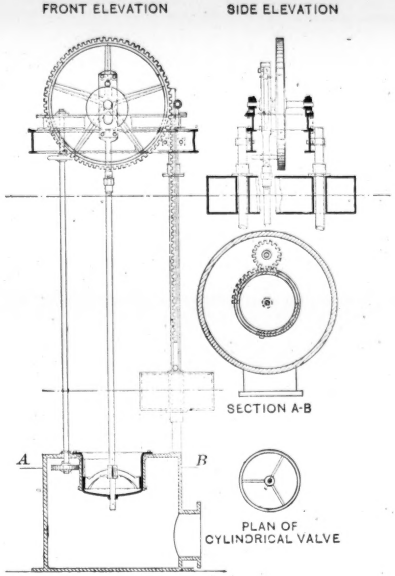


FIG. 9

Scale of Feet
0 1 2 6

the closing of a submerged orifice, or orifices, in two distinct directions, one automatic and differential, the other by hand to set to the discharge required. Fig. 8 shows the form in which the idea first occurred to the writer. It will be seen that the apparatus consists of two almost touching concentric cylinders, on opposite quadrants of each of which are cut two similar rectangular apertures, each occupying a little less than 90° of the circumference. The amount of opening is varied by the float, which causes the inner cylinder to rotate in the ratio of the square root of the head to which they are exposed. This is effected by means of a differential screw, planted or cut on the hollow central shaft, the development of which is part of a parabola. Fig. 9, which has already appeared in the correspondence on the City of Buenos Ayres Sanitary Improvement Works,* is the form which, for constructional reasons, best pleased the then Engineer in Chief of those works, Mr. Charles Nystromer.

It will be observed that the order of movements is the reverse of that which occurs in the first form, and that a cam has been substituted for the differential screw. It should be pointed out, however, that this arrangement, besides entailing greater mechanical complication, labors under the disadvantage of having more moving parts under water.

The form illustrated in Fig. 8 is not theoretically perfect, as in the adjustment for discharge the differential screw is lowered or raised in the guides of the float a distance twice as great as that traversed by the centroid of the apertures. Although this error, in all ordinary cases, is so small as to be negligible, it may be eliminated by either of the devices illustrated in Figs. 10 and 11; in the former by closing the orifices concentrically, in the latter by preserving constant the relative positions of the differential screw and of the centroid of the apertures.

As a matter of fact, however, the writer does not esteem these devices to be a real improvement on Fig. 8, which is his favorite form, since they entail greater complication, and inevitably give rise to an increase of friction.

As it may be objected that these modules would necessitate tedious calculation and difficult shopwork, in the shaping of the differential screw to meet each special case of change in the maximum and minimum depth of orifice, or range of water level, it may be pointed out

* *Minutes of Proceedings*, Inst. C. E., Vol. CXXIV, p. 86.

WITH ADJUSTABLE DISCHARGE.

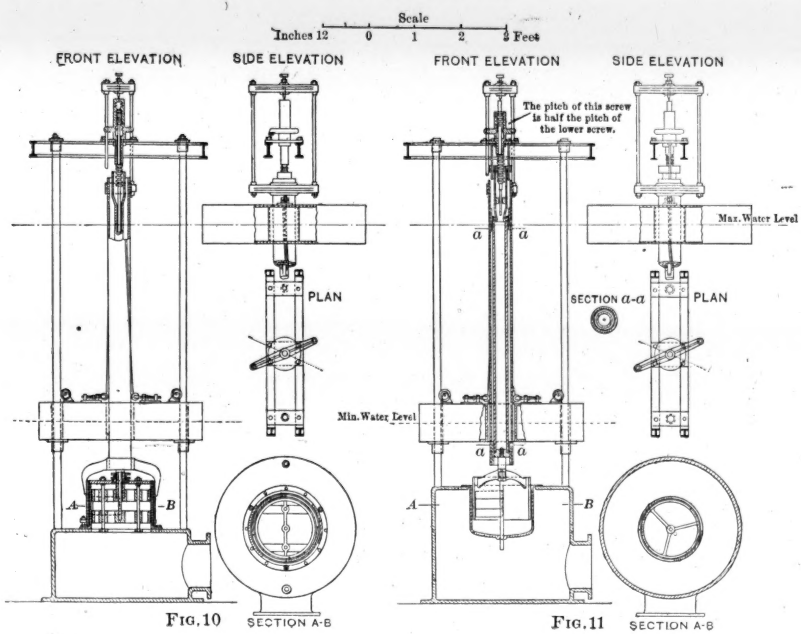


FIG. 10

FIG. 11

that it would suffice to draw out, once for all, the development of the screw (which is a parabola, having its vertex at the center of the aperture), and take that portion of it comprised between the maximum and minimum water levels of the special case under consideration. It will thus be seen that the construction of these modules is no more difficult than that of ordinary sluice-valves, and that, therefore, they can be manufactured and stocked like any other ironwork.

In conclusion, the writer hopes that the foregoing brief analysis of the various forms of automatic module which have come under his notice, or which he has evolved, may be found useful by members of the profession, when designing apparatus of a like nature.

DISCUSSION.

JOHN H. GREGORY, Assoc. M. Am. Soc. C. E. (by letter).—The author presents a paper on a subject which is of great interest to those who have had occasion to design apparatus for regulating the rate of filtration. He states, in the second paragraph: Mr. Gregory.

"Numerous forms of semi-automatic apparatus of this description have been devised, but as, under the law of the survival of the fittest, they appear to be doomed to speedy extinction," etc.

It is presumed that the author intends to classify under "semi-automatic apparatus" all apparatus which requires hand regulation, and if such is the case, the writer differs decidedly from the statement that such apparatus appears to be doomed to speedy extinction.

The author goes on to state that "at all events, for the purpose in view, it has not been considered expedient to include any special mention of them (semi-automatic apparatus) herein." The author's purpose was to describe automatic modules, as is revealed in the title of the paper, but the writer thinks that it would have been expedient to describe some of the forms of semi-automatic apparatus, point out the defects that may exist, and give reasons for not advocating them.

The author, possibly, is not acquainted with recent American practice in this line; but, if so, he could not have failed to note that in two of the largest and most recent plants, which it is hoped will be placed under construction in the near future, hand regulation has been adopted. The writer refers to the proposed slow sand filters at Pittsburg, the construction of which, unfortunately, seems to be postponed for some little time, and to the filters at Washington, D. C., contracts for which, according to the latest press reports, have just been awarded. While full details of these proposed plants have not been published, sufficient details have appeared in American engineering periodicals, in the last year or so, to show that hand regulation has been the type adopted. The writer knows of one other plant, which has been built during the last year, in which hand regulation is used. In another large plant which is to be built shortly, on the details of which the writer was asked to advise, the unanimous opinion of all concerned was in favor of hand regulation, and the details for the plant have been thus drawn. It would seem from this that, in the United States, hand regulation or semi-automatic apparatus is not doomed to speedy extinction.

The writer has had considerable experience in the last few years in designing apparatus of both automatic and semi-automatic types, and in watching the operations of both, and it may be of interest to point out some of the advantages and disadvantages of each.

While connected with the construction of the Albany filtration

Mr. Gregory

plant, designed and built by Allen Hazen, M. Am. Soc. C. E., he had the opportunity of watching the daily operation of the filters for several months. The apparatus at Albany, as is well known, consists of an orifice, 4 ins. high and 24 ins. long, in a fixed brass plate, situated in a cross-wall in the regulating chamber. The head on the orifice is maintained at a constant level by opening or closing a valve on the main effluent pipe from the filter. Suitable floats, with scales and markers, indicate the rate of filtration on the filter, corresponding to the head on the orifice. The apparatus was designed so that with a head of 1 ft. on the center of the orifice the rate of filtration would be 3 000 000 galls. per acre per 24 hours, and the orifice was placed so that the loss of head could reach a maximum of 4 ft. while still maintaining the head of 1 ft. on the orifice. The filters were designed to run at a normal rate of 3 000 000 galls. per acre per 24 hours, and no difficulty was experienced in throttling the valve on the effluent pipe so that the head on the orifice would be 1.00 ft., or, at the most, 1.01 ft. The variation in rate corresponding to these two heads is only a fraction of 1%, and is so small that it may be neglected.

After a filter is scraped, and when it is put in service, the loss of head is of course small, only a few hundredths of a foot; this increases gradually from day to day, slowly at first, but quite rapidly toward the last end of the run when the filter is clogged. The readings of the loss of head and rate of filtration are taken every few hours, and, if need be, the effluent valve is opened slightly to bring the rate of filtration back to the normal rate desired, as it drops off slightly during the period. This dropping off in rate is only a small amount, but, of course, is greater toward the last of the run; in both cases, however, it is not sufficient to affect the quality of the filtrate.

One possible objection to hand regulation in separate regulating houses may be raised, in that the opportunity is afforded the attendant to be negligent in his duties of regularly visiting the filters, and with little chance of detection. The larger the plant, the greater the objection might be; but the writer does not think that it is of sufficient importance to govern the selection of the type of apparatus to be used, except under certain abnormal conditions. He believes that it is as easy to obtain an attendant at a filter plant who will perform his duties faithfully as it is to obtain a watchman, who, in an entirely different kind of plant, will perform other duties faithfully.

If, now, instead of separate regulating houses, a single central regulating house is used, to which the effluent from each filter is piped, especially if the office is in the same building, the possible objection to hand regulation is removed. This latter is an arrangement which the writer has advocated for several years where the general lay-out of the plant is such as to admit of it.

Hand regulation, whether in single regulating houses or in a cen-

tral one, is advantageous in one respect, in that it requires periodical attendance at each filter, thereby ensuring that any irregularity will not pass long unobserved. With apparatus of the automatic type it is possible, of course, to lengthen materially the period between observations, in fact, with the apparatus set at a given rate to allow it to run until the loss of head is so great that the filter goes out of service. Under this extreme condition, if anything went wrong it would not be known. On the other hand, with satisfactory automatic apparatus, if for any reason the attendance for a short time is omitted, one can feel reasonably assured that things are going along all right. While hand regulation, from its nature, requires periodic attendance, the writer is decidedly of the opinion that automatic apparatus should receive just as careful attention, if the operation of the plant is to be above criticism. Mr. Gregory.

The author makes a very convenient classification of the various automatic modules, and, as would the writer, mentions the floating weir first, that being the type which would first suggest itself. In one of the figures, the author illustrates an apparatus of this type which he calls the Glenfield Jones, in which the discharge is regulated by varying the depth of submergence of the weir; and in commenting on this he states that:

"This, however, is an imperfect type, as friction causes the weir to be more or less submerged, depending on whether it is floating on rising or falling water, and, furthermore, the adjustment for varying the discharge is cumbersome and inconvenient."

As to whether or not apparatus of this type is imperfect, it would seem to depend almost entirely on the design of the details. If the sliding joint is arranged so that the friction is an appreciable quantity, the apparatus will be more or less imperfect; but experience seems to indicate that the friction at this point can be reduced to such a small amount that it can be neglected. As to whether or not the adjustment for varying the discharge is cumbersome and inconvenient, while it may be so with the apparatus illustrated, it can be said that this also depends entirely on the design of the details. The writer knows that it is possible to construct apparatus of this type so that the adjustment is accomplished in the simplest manner possible, that is, by turning a hand-wheel on an indicator stand.

While considering this subject, it may be of interest to illustrate three sets of apparatus of this type which were designed under the writer's direction for the slow sand filters at Philadelphia. The figures are only drawn in sufficient detail to indicate the general design and operation of the apparatus.

In Fig. 12 is shown the type which was first designed, and which is now in use at the six filters at the Lower Roxborough plant. This consists of a circular copper float, *F*, in the form of a torus, carrying above and secured to it a four-armed cast-iron spider, *Y*, through

Mr. Gregory. which is threaded a stem, *S*, at the hub, *A*. On the lower end of the stem, *S*, is carried a smaller four-armed spider, *K*, made of brass, so joined to the stem at *B* that it can turn freely in a horizontal direction, but without any vertical movement on the stem. This spider, *K*, supports a vertical brass pipe, *T*, which, for convenience, may be called the telescopic pipe, the top of which forms a sharp-crested weir, *W*, over which the water flows. This telescopic pipe is enclosed in a cast-iron casing, *C*, the lower end of which connects with the main pipe leading to the filtered-water basin. The telescopic joint, *J*, through which the pipe, *T*, passes, is a brass ring, and is arranged with grooves so that light packing could be inserted if desired. The stem, *S*, is of brass, the lower end only being circular and threaded; above, it is of square brass tubing, and passes up through a circular hole in the floor, which serves to guide the stem. Above the floor is placed an indicator stand with a hand-wheel. The stem passes up through this stand without being connected to it, the hub of the hand-wheel having a square hole through it slightly larger than the stem.

As shown, water is passing over the crest of the weir, thereby giving a certain rate of filtration on the filter. If it is desired to change the rate, this is accomplished by turning the hand-wheel on the indicator stand. By doing so, the stem, *S*, is turned, and, as it is threaded through the spider at *A*, the telescopic pipe, *T*, is raised or lowered with reference to the water level, correspondingly decreasing or increasing the rate. In order that the float, *F*, shall not turn with the stem, a small projecting piece is fastened to it, moving along the guide, *L*, which is fastened to the wall.

The rate of filtration is indicated by a pointer moving over a small horizontal dial at *G*, directly under the hand-wheel. The movement of this pointer is accomplished by means of a brass collar, with a square hole, fitting around the stem, and forming the worm of a train of gearing. The graduation of the dial is determined from experiment rather than from computation.

The weak point in apparatus of this type is, of course, at the joint, *J*. When it came to designing this joint, three styles were considered, one being that shown, another a thin plate with a hole very slightly larger than the telescopic pipe, and the other a cup leather. The writer rather leaned toward the leather joint or the thin plate, but after discussion it was finally decided to adopt the one shown, with the provision, however, that the design should be such that either of the others could be substituted if it was found desirable. It should be remarked that with either of the types which do not make a tight joint at *J*, there will be a certain amount of leakage, greater, of course, at the first of the run when the head is greater; but, in the writer's opinion, this leakage would be so small, compared with the amount

Mr. Gregory.

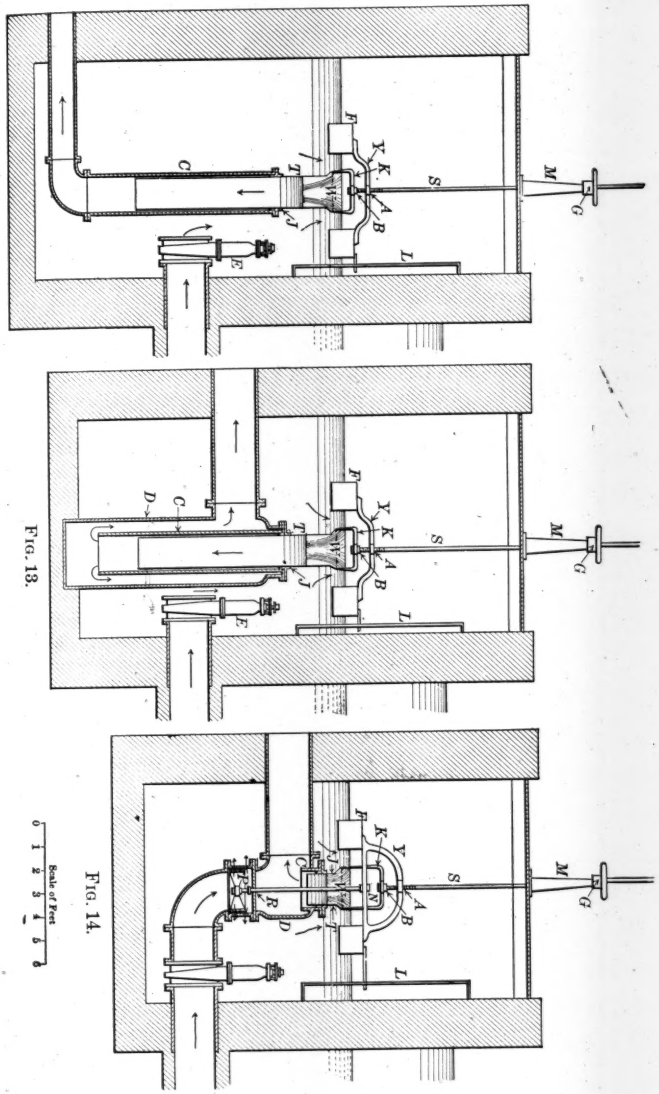


FIG. 12.

FIG. 13.

FIG. 14.

Scale of Feet.
0 1 2 3 4 5 6

Mr. Gregory.

passing over the weir, that it would not be a serious matter. This leakage, however, is to be avoided if possible.

After this apparatus was installed it was found that with the joint as designed it did not operate satisfactorily. The writer understands that the cup leather has now been substituted and is giving very satisfactory results.

In designing both sets of apparatus it was intended to provide access for air under the sheet of water, and it was thought that sufficient opening had been provided at the four points where the spider, *K*, was joined to the pipe. It was expected, however, that there would be some entrained air carried along in the solid column of water passing down through the telescopic pipe, due to the agitation at the surface produced by the falling sheet of water. In either event it was not anticipated that it would amount to much. It seems that such, however, was not the case, and that there has been some little trouble caused by air being sucked in. The writer has not had the opportunity of studying the apparatus for any length of time while in operation, and hence cannot suggest what he would consider the best way of remedying it; but, from his knowledge of all the details, several methods have suggested themselves. The trouble with the air in the water passing out through the apparatus, however, does not affect the satisfactory operation of the filter.

When it came to designing the Upper Roxborough and Belmont plants it was found desirable to raise the elevation of the effluent pipe leading from the regulating chambers. This resulted in a modification of the design of the regulating apparatus, which modification is shown in Fig. 13. The upper part of the apparatus is the same as that just described. The change in the lower part is indicated clearly in the drawing. The object in providing the internal casing, *C*, was to prevent a possible cramping of the telescopic pipe, *T*, in the joint, *J*, due to the lateral movement of the water as it passed out into the effluent pipe leading from the chamber. This modified type of apparatus was also included in the designs of the Torresdale plant, to be used for all the filters except one, in which was to be installed a still further modified type.

All the apparatus was designed to run for a maximum rate of filtration of 6 000 000 galls. per acre per 24 hours, and with a maximum loss of head on the filter of 5½ ft. It is readily seen, then, that when the filter is first started after scraping, the water passing over the weir falls several feet, causing the agitation mentioned previously. It should also be noted that during the run the telescopic pipe has a vertical movement corresponding to the total loss of head, and a consequent variation in leakage if a tight joint is not used at *J*, as before stated. It was in an attempt to improve both these features that the apparatus illustrated in Fig. 14 was designed.

Mr. Gregory.

The indicator stand, the stem, *S*, the joints, *A*, *B* and *J*, and the float, *F*, are the same as described previously. The spider, *K*, is lengthened vertically, but has no connection with the spider, *Y*, except through the stem, *S*, although the drawing might seem to indicate otherwise. The spider, *Y*, is a double one, the upper part being similar to the one described previously, and serving the same purpose; the lower part carries a vertical valve stem at *N*. The telescopic pipe, *T*, is made much shorter, while the outlet casting, *D*, is quite different in form. Below this outlet casting, *D*, there is a solid plate with an opening at *R* for the valve stem, and below the plate there is a balanced piston-valve, placed on the end of the main effluent from the filter. This valve consists of a hollow internal cylinder or piston, *P*, with openings, surrounded by an annular casing, also with openings. The position of the piston, *P*, is controlled by the float, *F*. The method of changing the rate of filtration, as with the other types, is by changing the level of the crest of the weir, *W*, relative to the water level in the chamber.

The operation of the apparatus may be described as follows: When the filter is started, after scraping, the piston, *P*, will be at its lowest position, with its openings opposite those in the annular casing. The water will pass out through the piston-valve into the chamber, rising up around the float and passing over the weir. When the water has reached a sufficient elevation, the float will rise, carrying with it the piston, *P*, and thereby partly closing the openings in the annular casing. The float will rise to such a position that the water discharged through the piston-valve will be the same as that passing over the weir, in which position the apparatus will be in equilibrium. The difference in head between the water level on the filter and in the chamber, minus the head lost in passing the filter, will be used up in passing through the piston-valve. At the beginning of a run this head will be several feet, but at the last end will be only a small amount. If at any time the rate is changed, the position of the float and piston will be altered slightly to correspond to the new condition of affairs.

It will be seen, then, that with this type of apparatus the water level in the chamber will remain practically constant, varying at the most only a few inches instead of fluctuating several feet, and thereby giving practically a constant leakage at *J*, if a tight joint is not used. The telescopic pipe, *T*, will have a vertical movement of only a few inches, and its length has been reduced to a minimum. A cup-leather joint, preferably, would be used at *R*, to prevent leakage at this point.

The writer wishes to acknowledge his indebtedness to Mr. L. M. Lloyd and Mr. F. C. Dunlap, his assistants, for several valuable suggestions in working out the details of the three types of apparatus just described.

Mr. Gregory. In conclusion, the writer would add to the two requirements which the author points out should be fulfilled by automatic apparatus, a third, namely, that the loss of head caused by the apparatus should be reduced to a minimum. In the Philadelphia designs it was this requirement which was the determining factor in deciding the use of the floating weir in which the submergence of the weir could be varied.

Mr. Copeland. W. R. COPELAND, Assoc. Am. Soc. C. E. (by letter).—In his discussion on automatic modules, Mr. Gregory describes the automatic weirs which are used to regulate the rate of filtration at the Roxborough filters in Philadelphia.

By glancing at the diagrams, Figs. 12, 13 and 14, it will be seen that the weirs have the following points in common: They are supported by floats which rise and fall with the level of the water in the filtered-water chambers; they consist of pipes which are held in a vertical position by spiders attached to the floats; the lower part of the weir pipe slides up and down in a casing, and for that reason it is designated a "telescopic" pipe; the water in the filtered-water chamber escapes from that chamber by running over the edge of the weir, passing down through the telescopic pipe and out into the effluent mains.

The principal differences in the designs of the "regulators" are as follows: At Lower Roxborough the effluent mains are fastened directly to the bottom of the casing in which the telescopic pipe slides, whereas at Upper Roxborough the effluent pipe leads off from the side of the casing; moreover, the water drops farther at Lower than at Upper Roxborough.

In falling, the column of descending water creates a partial vacuum. In order to prevent this, air rushes into the telescopic pipe, both at the center and behind the sheet of water. A portion of this air mixes or becomes "entrained" with the water, and, under certain conditions, passes out through the bottom of the telescopic pipe into the effluent mains.

Commenting upon this feature of the action of the automatic weir, Mr. Gregory says:

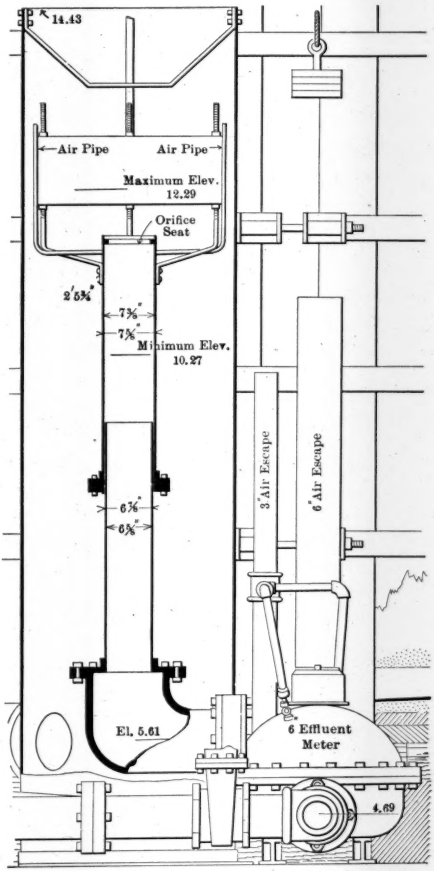
"It was expected, however, that there would be some entrained air carried along in the solid column of water passing down through the telescopic pipe, due to the agitation at the surface produced by the falling sheet of water. * * * It seems * * * that there has been some little trouble caused by air being sucked in."

The "trouble" to which he refers arose from the formation of a water hammer which was so severe that a large valve on the outlet of the filtered-water main had to be taken off, in order to prevent the "hammer" from breaking the pipe joints. This is not the first time that trouble has been caused by automatic regulators of these general designs. In 1898 the Filtration Commission, of Pittsburg,* made

* "Report of the Filtration Commission," Pittsburg, 1899.

some experiments in water filtration with a mechanical filter of the so-called "Warren" type. Mr. Copeland

As at Roxborough, the Warren regulator consisted of a float supporting a weir, but in this case the weir was made of an "orifice" plate set in the top of the telescopic pipe, and the water entered the pipe through this orifice. Air was sucked in by the sheet of falling water at the center, and through air-pipes tapped into the top of the telescopic pipe just below the orifice plate. See Fig. 15.



WARREN AUTOMATIC WEIR

FIG. 15.

The column of water falling downward sucked in air to a considerable degree, as is shown in the following statement:*

"It was found necessary to allow an abundance of air to enter the top of the central pipe of the automatic weir, to prevent a vacuum from being formed and thus cause the float and orifice to drop, causing too much water to flow. The entrance of the air, however, caused serious over-registration of the (water) meter upon the effluent pipe; the error occasioned in this way reaching 13% of the volume of water filtered. After a trial of various devices, vents were arranged which reduced the error to 4 per cent."

The vents referred to are shown on the right of Fig. 15,† and were three in number. The largest, 6 ins. in diameter, was placed on top of an elbow in the effluent pipe just before the

* "Report of the Filtration Commission," Pittsburg, p. 158.
† "Report of the Filtration Commission," Pittsburg, Plate No. 16.

Mr. Copeland.

meter. The other vents were smaller and were placed on top of the meter in order to release the air which passed by the 6-in. vent.

Although some air was sucked in through the air-pipes, the volume was small. It produced a vacuum of about 2 ins. on a mercury column. Therefore the air entrained at the central well created most of the trouble.

The volume of air entrained by an automatic regulator will depend upon several factors.

As the air is sucked in because of the downward draft caused by the falling sheet of water, the distance which the water falls is an important factor in the process of entraining air. Advantage is taken of the fact that the "draft" varies with the distance which the water falls in developing power by turbines.

The manner in which the water is drawn away from the bottom of the telescopic pipe is another important factor. If, for instance, the telescopic pipe discharges directly into an effluent main which has considerable slope, or upon which so much draft is made that the water is "drawn" out of the lower part of the casing with great rapidity, the pipe will be emptied to the very bottom. In consequence of this, air, forced into the telescopic pipe by the downward rush of the falling sheet of water, will be driven into the effluent main.

But, if the telescopic pipe discharges into a chamber which is arranged in such a manner that the effluent main does not draw the water away immediately from the telescopic pipe, the downward current will be checked before the water enters the main. As a result, the level of the water will tend to rise in the telescopic pipe, shortening the fall and preventing the entrained air from being forced into the main to a considerable degree.

TABLE No. 2.

Regulator designed for:	Maximum distance through which the water falls (in feet).	Rate of flow through the telescopic pipe. Linear feet per second.
Warren Filter	6	0.23
Lower Roxborough	15	0.31
Upper Roxborough	7.7	0.41
Torresdale (Special)	2	0.41

The volume of air entrained by the Warren filter, as pointed out in a former paragraph, was measured accidentally. The air entrained by the Roxborough regulators has never been measured, but, in order to give an idea of the probable action of these weirs, data prepared for the Warren and Roxborough filters have been placed in Table No. 2 for comparison.

From the figures given in Table No. 2 it is evident that the

"length of fall" and the "rates of flow" are greater at the Rox- Mr. Copeland
borough filters than they were for the Warren filters. Therefore, it would be natural to expect that the volume of air entrained would be greater at Roxborough than at Pittsburg. The air entrained at Lower Roxborough is so considerable in volume that the theoretical deduction seemed to be borne out in practice. But, at the time of writing, no such results have developed at Upper Roxborough. These filters, however, have not yet been given a test at their maximum "working" rate of filtration.

By comparing the diagrams of the Warren and Lower Roxborough filters, one notes that the methods of drawing the water away from the telescopic pipes are the same, namely, by direct discharge into the effluent mains. The telescopic pipe at Upper Roxborough discharges into an "outlet casting," *D*, and the effluent main leads off from this casting at a point several feet above the point of discharge.

Whereas, then, the water is taken away from the Lower Roxborough telescopic pipes by what may be called "direct" discharge, the water is taken away at Upper Roxborough by what may be called an "indirect" or "trapped" discharge.

Instead, therefore, of forcing the entrained air directly into the effluent main, as at Lower Roxborough, the trap at Upper Roxborough gives the air a chance to separate from the water before it enters the effluent main.

Moreover, no matter at what rate of filtration the lower system may be operating, the steep slopes and arrangement of the effluent mains will always draw the water away to the bottom of the outlet casting; but, when the filters run at low rates of filtration at Upper Roxborough and the filtered-water reservoir is full, the traps will "back" water up in the telescopic pipes of the regulators until the fall is shortened to only about 2 ft.

As the experience at Upper Roxborough, up to the present time, has indicated that with a fall of 2 or 3 ft. only, a small volume of air will be entrained, it is probable that the special form of regulator designed for Torresdale will reduce, to a considerable degree, the quantity of air entrained.

The diagram of the special regulator shows, however, that the telescopic pipe is very short, and that the effluent pipe occupies the larger part of one side of the outlet casting. Therefore, the discharge from this regulator is direct, and any air entrained by the column of falling water will be forced into the effluent main, just as it is at Lower Roxborough.

As Mr. Gregory states, the entrained air does not affect the efficiency of the filters. In fact, Mr. John W. Hill suggests that the aeration which this air will produce may be a benefit. But the action of the air on the valves and pipe lines is a danger which can be removed only by getting rid of the air.

Mr. Copeland. Part of the air can be released by vents, but the experience with the Warren filter at Pittsburg showed that the escape pipes having an area greater than the effluent main failed to remove all the air entrained by the Warren automatic regulator.

Therefore, it is probable that vents placed on the top of the "outlet casting, *D*," would remove part but not all of the air entrained by the weir. In order to get rid of the remainder of the air, it seems to the writer that the diameter of the first length of the outlet main should be increased to double its present size, and a large vent placed on top at the outlet end.

By increasing the diameter of the pipe the rate of flow through the pipe would be decreased, and a better chance be given for the air to separate from the water.

The entrained air escapes from the water just as soon as it reaches the clear-water reservoir. But if it should ever become necessary to "by-pass" the reservoir, delivering the water directly into the city mains, it is probable that the air entrained by the automatic floating weirs might break some of the joints in the pipes.

In order to prevent this, the floats can be forced under water until the weirs are submerged deep enough to prevent air from being sucked in at the center of the regulators.

When the weirs are submerged in this manner no way is left to control the rate of filtration except by opening the hand-valves a known number of turns. Even then the rates are likely to fluctuate directly with the variation in the consumption of the water. As irregular rates of filtration are likely to interfere with the processes of purification, it is very desirable to install regulating devices which will reduce "personal superintendence" as much as possible.

Therefore, if controllers can be made which will run automatically for twelve or fifteen hours, and if, as Mr. Gregory suggests, the controllers are brought from several filters into one house, the regulation of the rates of filtration can be kept in the hands of the engineer in charge of the plant.

Mr. Hill. JOHN W. HILL, M. Am. Soc. C. E. (by letter).—The experience with plain sand filtration in Philadelphia, at the testing stations located on the Schuylkill and Delaware Rivers, and at the Roxborough filters, indicates that it is extremely desirable that filters should operate at a uniform rate of percolation per unit of area per day. Oddly enough, it is sometimes shown that a rapid reduction of the rate of filtration is accompanied by an increase in the bacterial content of the effluents of the filters, and that at times when the rates are increased materially the bacterial content diminishes with the increased rate. These are phenomena which are still to be investigated to determine the true causes of what would appear to be anomalous results.

There is one serious defect in the floating circular weir, used to

regulate and measure the rate of flow from the Philadelphia filters, as pointed out in one of the discussions on this paper, *viz.*, the very large quantity of air carried into the effluent pipes by induction or suction of the annular column of water flowing down the telescoping weir pipe. Mr. Hill.

All the plans for the Philadelphia filters contained, on the end of the effluent pipe, where it enters the clear-water basin, an automatic balanced valve, intended to adjust the rate of flow from the filters to the clear-water basin in such a manner that variations in the elevation of the water in the basin would cause corresponding variations in the rate of discharge from the filters (and gradually increase or diminish the rate of flow from the filters), to adapt the work of the filters to the requirements of consumption; but the large quantity of air carried down the telescoping effluent weir pipes rendered impossible the use of the regulating balanced valve at the clear-water basin, as intended. Steps are now being taken to substitute for the floating weir an adjustable submerged orifice, the depth of submergence of the orifice remaining constant while the area of the opening will be adjustable to the varying rates of flow of the filters. It is expected that the change, in this direction, will avoid the introduction of the large volume of air which now passes through the effluent pipe along with the water, and not only interferes with the proper operation of the main effluent regulator, placed at the end of the effluent pipe in the clear-water basin, but, to a material extent, diminishes the capacity of the effluent pipes, and causes a greater loss of head, in effecting the discharge of the filtered water from the filters, than is desirable.

It is not known that the induction of air constitutes any other disadvantage than the one mentioned. If it were not that the air is in contact with the water in the effluent pipe for so short a time it might be said that the aeration of the filtered water flowing over the lip of the weir would be an advantage; certainly it could not be considered a detriment, although in this manner it is possible that air organisms may be drawn into the water and appear in the cultivation of samples of water taken from the clear-water basin for analytical purposes.

With reference to an automatic module, or effluent regulator, there is no doubt of the desirability of having a device which will adjust itself to the increasing loss of head in the operation of the filters, and maintain a constant discharge of water from the filters, but, unless such a device is absolutely reliable, there is a question whether the adjustable weir at Berlin, or the adjustable orifice used in the Hamburg and in the Albany filters, would not be preferable to the automatic regulator, the former compelling attention upon the part of the filter attendants, and a constant adjustment of the weir or orifice to the fixed rate of operation of the filter.

Mr. Hill. In the original form of the Lindley floating weir used in the Warsaw filters, the same trouble was found that is now being found with the Philadelphia effluent regulators, and recent information from Mr. I. Lindley, of Warsaw, shows that the original form of automatic regulators has been superseded by a form containing an adjustable orifice placed below the surface of the water, with more satisfactory results in the operation of the automatic regulators.

It is the practice, in the works of the East London Water Company, to regulate the flow of water to and from the filters by valves adjusted by hand; but the rate of percolation is influenced directly by the draught of water by the pumps, pumping into the distributing reservoirs from the small clear-water basin into which the effluents from the filters are discharged.

The packing designed for the telescoping tubes of the Philadelphia effluent regulators was first a plain stuffing-box, and later a water-packed bronze gland through which the telescoping tube moved up and down. It was found that the leakage, however, when the packing was adjusted for the free movement of the telescoping tubes, was too great to admit of considering the automatic weir as a device for the measurement of the rate of flow from the filters, and in due time the original packing devices were removed and self-adjusting cup-leather packings substituted, which now allow the floats to respond promptly to slight changes in the water level in the effluent chambers, and maintain a reasonably constant head on the circular weir at the top of the telescoping tube.

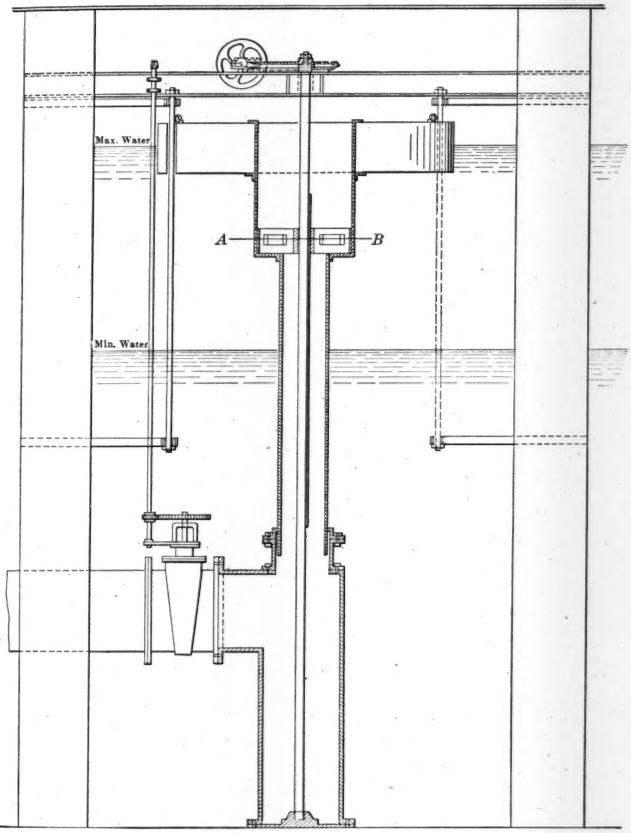
The flow over the weir, for any adjustment, is determined very accurately by stopping the flow of water into the filters, and noting the decline of head over the sand bed for a period of twenty minutes or half an hour, from which loss of level, and the known area of the filter tank, the rate of percolation is determined, and the weir rated for discharge accordingly. Such measurements are made conveniently for each adjustment of the head on the weir, and as long as the depth of flow over the lip of the weir remains unchanged, the weir may be accepted as a reliable means of measuring the discharge of the filter, as well as maintaining a constant rate of percolation through the sand bed.

Mr. Anthony. CHARLES ANTHONY, Jr., M. Am. Soc. C. E. (by letter).—In answer to Mr. Gregory's observation, the writer may say that he selected the Glenfield-Jones form of weir module as a typical example of its class, neither on account of the excellency or otherwise of its design, nor on grounds of originality—Mr. Lindley having been the first, he believes, to construct regulators of this kind – but because it is one of the few modules actually manufactured as an article of general commerce.

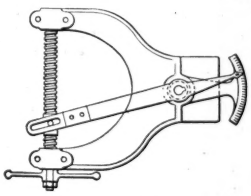
The form of module of the same class illustrated by Mr. Gregory,

AUTOMATIC MODULE.

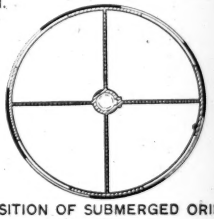
Mr. Anthony



ELEVATION.



PLAN OF REGULATING GEAR.



DISPOSITION OF SUBMERGED ORIFICES.
SECTION *A-B*.

FIG. 16.

Mr. Anthony. in Fig. 14, has many good points, not least of which in many cases would be the small loss of head involved; *apropos* of which, the writer may point out, that while agreeing that the smallness in the loss of head caused by an apparatus of this nature is often of the highest importance, this requirement can hardly be classed among the fundamental ones formulated.

The difficulty of entrained air, referred to by Messrs. Gregory, Copeland and Hill, has likewise been met by the writer, but it can be diminished, as pointed out, by a disposition of the apparatus which reduces the height of fall in the vertical tube. However, it is on this account, as well as on the grounds of the greater uniformity of flow obtained by substituting a submerged orifice for a surface weir, coupled with the easy manner of varying the rate of flow, which have led the writer to prefer the type illustrated in Fig. 2 to the original surface-weir form. And he is interested to hear from Mr. Hill that both at Warsaw and Philadelphia the regulators adopted finally were of this type.

In Fig. 16 is shown, in elevation and plan, the form of this class of module which has recently been introduced into the filtering plant of the City of Buenos Aires. As will be seen, it differs from the form illustrated in Fig. 2 only in points of detail, and in the greater refinement of the method by which the adjustment for varying the discharge is effected.

While admitting that recent American practice, as Mr. Gregory indicates, has been in the direction of a reversion to semi-automatic regulation, the writer cannot but think that with the perfecting of automatic modules, the day cannot be far distant when automatic regulation will be the rule and not the exception, at all events, in all plants with any pretensions to completeness.

The present atavistic tendency he considers is due, not to any inherent advantage in hand over automatic regulation—the contrary being the case—but to the fact that all automatic modules hitherto tried have been more or less defective. And if his brief paper and the interesting discussion to which it has given rise have in any way forwarded the evolution of the required apparatus, he is well satisfied.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

Paper No. 962.

TESTS OF THE EFFICIENCY OF HOISTING TACKLE.*

By S. P. Mitchell, M. Am. Soc. C. E.

For the purpose of gaining information as to the efficiency of hoisting tackle, an extended series of tests was made recently at the Pencoyd plant of the American Bridge Company, with standard types of blocks, and various combinations of manila and wire rope. The tests were arranged so as to reproduce, as nearly as possible, actual conditions under which hoisting tackles are used in the field.

An eye-bar testing machine, of 300 tons capacity, and a vertically-moving hydraulic crane, were used in making the tests. One block was attached to the fixed head and one to the movable head of the machine, the latter having a horizontal motion, under constant pressure, of about 6 ft. The line was led, as shown in Fig. 1, from the movable block to a snatch block at the fixed end of the machine, thence, with a 180° turn, to a second snatch block, thence, with a 90° turn, to a vertically-moving hydraulic crane, which furnished the lead-line pull.

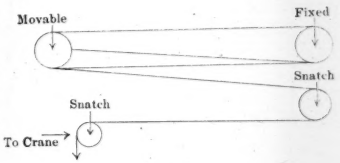


Fig. 1.

The load registered on the testing machine was thus carried on as many parts of line as ran to the movable block, this number being one in excess of that leading to the fixed block.

* Presented at the meeting of September 16th, 1903.

The load on the machine, and the pull exerted by the crane, were taken from their respective mercury pressure gauges, upon which many simultaneous readings were made for each individual test. Separate readings, of course, were taken to determine the friction of the testing machine and crane.

Each combination of tackle was tested in lifting, *i. e.*, with the movable block approaching the fixed block, as in lifting a load; and for a becket pull, *i. e.*, with the movable block receding from the fixed block, as in lowering a load. The results of these tests are shown in Table No. 1. The tested efficiencies there given are derived as follows: If W be the load on the machine, n the number of parts of line carrying the load, and P the pull of the crane, the tested efficiency for straight lifts is $\frac{W}{n\,P}$.

For the becket pulls, in which the machine is pulling against the crane, an analogous value for efficiency is $\frac{n'\,P'}{W'}$; where W' is the pull of the machine, n' the number of parts of line, and P' the resistance of the crane. It should be noted that the results of the individual tests, as given in the column of tested efficiencies, are each derived from a large number (in most cases forty to fifty) of gauge readings.

Under field conditions, a lead-line is frequently snatched a number of times before reaching the engine. It was desired, therefore, to obtain results of the tests, free from the influence of the two snatch blocks used, and involving only the parts of the line holding the load, of which the first should be considered the lead-line, as shown in Fig. 2.

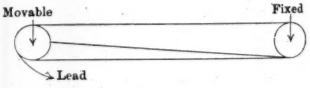


FIG. 2.

For this purpose the efficiencies of the combinations tested were worked out according to the method given by Weisbach.*

The formula there used for the efficiency of a fixed pulley is

$$\frac{1}{K} = \frac{1}{1 + C\frac{d^2}{r} + 2\ \sigma\frac{P}{r}\sin.\ L}$$

In which d = diameter of rope;

r = effective radius = distance from center of pulley to center of rope;

* "The Mechanics of Hoisting Machinery." Translated from the second German edition by Karl P. Dahlstrom, M. E.

TABLE No. 1.—EFFICIENCY OF TACKLE WITH TWO SNATCH BLOCKS.

	Parts of line.	Tested Efficiency: Percentage.				Efficiency, from Formula.	
		Lifting.	Mean.	Becket pull.	Mean.	Lifting.	Becket pull.
1¼-in. manila line. C = 0.46.	4	60, 60, 56, 57, 55, 53	57	37, 42, 50, 56, 57, ..	48	60	58
	6	52, 51, 53, 52, 52	52	56, 50	53	53	53
	7	44, 48, 52, 51	49	41, 46	44	50	45
1¼-in. manila line. C = 0.40.	4	52, 48	50	47, 50	49	56	55
	7	46, 46	46	42, 43, 43	43	47	42
	9	48, 48, 56, 52, 52, 43, 43, 43, 41	47	40, 37, 38	38	42	36
1¾-in. manila line. C = 0.38.	5	45, 47, 44	45	48, 49, 49	49	47	45
	7	39, 42, 41	41	43, 33, 42	40	42	37
	9	37, 41, 39	39	34, 33, 36	34	37	31
2-in. manila line. C = 0.34.	7	39, 41, 44	41	32, 38, 37	36	40	36
	9	36, 38, 39	37	37, 37, 39	38	35	30
¾-in. wire rope. C = 1.8.	3	47, 48, 46	..	42, 47, 40, 35	....	....	....
	5	49, 48, 49	49	42, 41, 42	41	....	....
	7	49, 50, 49	49	46, 45, 41	44	55	56
	9	53, 58, 55, 49	54	41, 44, 43	43	50	49
	13	43, 44, 48, 46	45	53, 51, 51, 54	52	42	37

TABLE No. 2.—Capacity and Efficiency of Tackle without Snatch Blocks.

	Parts of line.	Lift per unit pull in lead-line.	Efficiency: Percentage.
1¼-in. manila line	2	1.92	96
	3	2.68	89
	4	3.37	84
	5	3.95	79
	6	4.48	75
	7	4.92	70
1½-in. manila line	2	1.91	96
	3	2.67	89
	4	3.36	84
	5	3.93	79
	6	4.45	74
	7	4.89	70
	8	5.28	66
	9	5.61	62
1¾-in. manila line	3	2.64	88
	4	3.30	83
	5	3.84	77
	6	4.33	72
	7	4.72	67
	8	5.08	64
	9	5.37	60
2-in. manila line	5	3.87	77
	6	4.37	73
	7	4.78	68
	8	5.14	64
	9	5.45	61
¾-in. wire rope	3	2.73	91
	4	3.47	87
	5	4.11	82
	6	4.70	78
	7	5.20	74
	8	5.68	71
	9	6.08	68
	10	6.46	65
	11	6.78	62
	12	7.08	59
	13	7.34	56

TABLE No. 3.—Dimensions of Blocks Used in Tests.

Metaline bushed.	Center of sheave pin to tread of sheave.	Diameter of sheave pin.
12-in. blocks for 1¼-in. manila line	3¼ ins.	⅞ in.
14-in. blocks for 1½-in. manila line	3⅝ "	1 in.
16-in. blocks for 1¾-in. manila line	4⅛ "	1⅛ ins.
20-in. blocks for 2-in. manila line	5⅛ "	1⅜ ins.
Snatch blocks for manila line	3⅝ "	1 in.
Wire-rope blocks	7 "	2⅛ ins.

P = radius of sheave pin;

$2\,L$ = arc of contact of rope on sheave;

σ = coefficient of journal friction = 0.08;

C = empirical constant.

The efficiency of the movable pulley, for forward motion, or lifting, is $\frac{1+K}{2\,K}$.

For reverse motion, or becket pull, it is $\frac{2}{1+K}$.

It was found that a constant value of C did not give computed efficiencies agreeing sufficiently well with those of the tests. Accordingly, different values of C were selected for the different sizes of rope. These are given in Table No. 1, together with the efficiencies computed from the formula.

The constant, C, for $\frac{3}{4}$-in. wire rope, was derived from the tests on 7, 9 and 13 parts of line, as the friction of the testing machines in the tests on 3 and 5 parts of line was so large, in proportion to the load lifted, as to render the results of the two tests of doubtful value.

Table No. 3 contains the essential dimensions of the blocks tested.

Having determined a suitable value of C for each size of line, it only remained to compute the efficiencies of the several combinations, omitting the snatch blocks.

The results are shown in Table No. 2, together with the lift of each combination per unit pull in lead-line; the lead being, as explained previously, the first part of the line holding the load.

The tests were in charge of J. L. de Vou, Assoc. M. Am. Soc. C. E., Assistant to the Chief Engineer, assisted by Mr. S. L. Wonson, Assistant Engineer.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INSTITUTED 1852.

TRANSACTIONS.

Paper No. 963.

THEORY OF CENTRIFUGAL PUMPS AND FANS: ANALYSIS OF THEIR ACTION, WITH SUGGESTIONS FOR DESIGNS.*

By Elmo G. Harris, M. Am. Soc. C. E.

With Discussion by Messrs. William Mayo Venable, E. T. Adams, Allen Hazen, Joseph Mayer, Theodore Horton and Elmo G. Harris.

Introduction.

The writer recently had occasion to look for a satisfactory discussion of the theory of centrifugal pumps, and, not finding the subject introduced at all in most of the books on kindred subjects, he undertook to hunt up some special literature bearing upon it, and was surprised to find that, apparently, the subject of centrifugal pumps had been overlooked or neglected by competent writers on applied mechanics.

That this is true is a matter of no small surprise when the importance of the subject and its attractiveness, theoretically, are considered.

To those who would say that to follow out the conclusions recommended in this paper would lead to unjustifiable refinement in machines applied to rough and temporary use, the answer should be: Knowledge of correct principles does not necessitate extravagant

* Presented at the meeting of September 16th, 1903.

refinement, but with it we can simplify and cheapen the machine in such a way as to make the least sacrifice of efficiency; and, further, that there are no apparent reasons why refinement in proportions and workmanship will not yield results fully justifying the extra cost, thus placing the machine in a higher class and extending its use to conditions now considered out of its field.

As a result of this study there are proposed three classes of pumps—Centrifugal, Impulse and Turbine. The necessity of distinguishing purely impulsive action from centrifugal force will be apparent to anyone who follows the discussion, and who has a fair knowledge of mechanics.

In the mathematical equations and formulas introduced in this study there is no attempt to include a factor or term to account for losses by friction, impact, or otherwise. Such losses will occur, of course, but they are of a nature which does not permit of mathematical formulation. The writer thinks it better to present the underlying principles clearly, with the simplest and least possible mathematics, and then interpret the results of experiment by comparison with what ought to occur under perfect conditions. With this in view, the several causes of loss are discussed, and suggestions offered for finding where the greater portion of this loss occurs. (See page 177.)

The writer has found some attempts at mathematical analysis for determining the proportions of a pump that should give maximum efficiency, but thinks that such analyses are a misuse of mathematics, and are apt to put the science in disrepute. Whatever the design of a centrifugal pump (or of an impulse pump), the dynamic principle on which it acts is that of imparting energy to water by giving it a high velocity. Within the machine, the water must be taken from a state of rest (practically), put in motion and brought to rest again, or nearly so. The problem for the designer is then, simply: To put water in motion and stop it again with the least possible friction of water on metal and of water on water—simple in statement, but not simple in accomplishment.

In taking up this study the writer had no intention of going beyond the mathematical analysis, but, was irresistibly led into attempts at designing with a view to reducing friction losses to a minimum. In these designs the novel features introduced are: First, the introduction of air instead of water between the wheel and casing; second, the

support of the wheel (in some designs) by air pressure from below; third, the use of flaring nozzles for checking the velocity of the water as it leaves the propellers, whereby the energy of the velocity can be converted into pressure with the least possible loss; and, fourth, making the discharge adjustable.

With pumps designed and tested according to the conclusions of this study, it will be possible to determine satisfactory coefficients to apply to pumps of a specific class, working under specific conditions.

The proposed modifications, in construction, in proportion, and in operation, make machines so different from anything yet tried that they must be considered as designs evolved from theoretic considerations without the light of experience. Though all the theoretic results may not be realized to a satisfactory degree, the writer believes the study will result in improvements on present practice.

Centrifugal Pumps.

Symbols.—For convenience of reference, the symbols used are collected here. The reader should not be discouraged by the formidable array; they do not all come in at once, and the working formulas will be found to be surprisingly simple. Most of the symbols are also indicated on the diagram, Fig. 1.

r = Radius of outer limit of vanes;

r_1 = Radius of inner limit of vanes;

x = Radius of any intermediate point—a variable;

u = Velocity of revolution of outer limit of vanes;

u_1 = Velocity of revolution of inner limit of vanes = $\frac{r_1}{r} u$;

V = Velocity of escaping water, relative to revolving wheel;

V_1 = Velocity of entering water, relative to revolving wheel;

v = Absolute velocity of escaping water;

v_1 = Absolute velocity of water as it enters between vanes;

W = Weight of a cubic unit (foot) of the water (or other liquid);

b = Width of vanes at outer limit = width of circumferential opening or slot through which discharge occurs;

b_1 = Width of vanes at inner limits;

a = Area of discharge slot = $2 \pi r b$;

a_1 = Area of entrance to vanes = $2 \pi r_1 b_1$;

Q = Quantity (cubic feet) of liquid passing (per second);

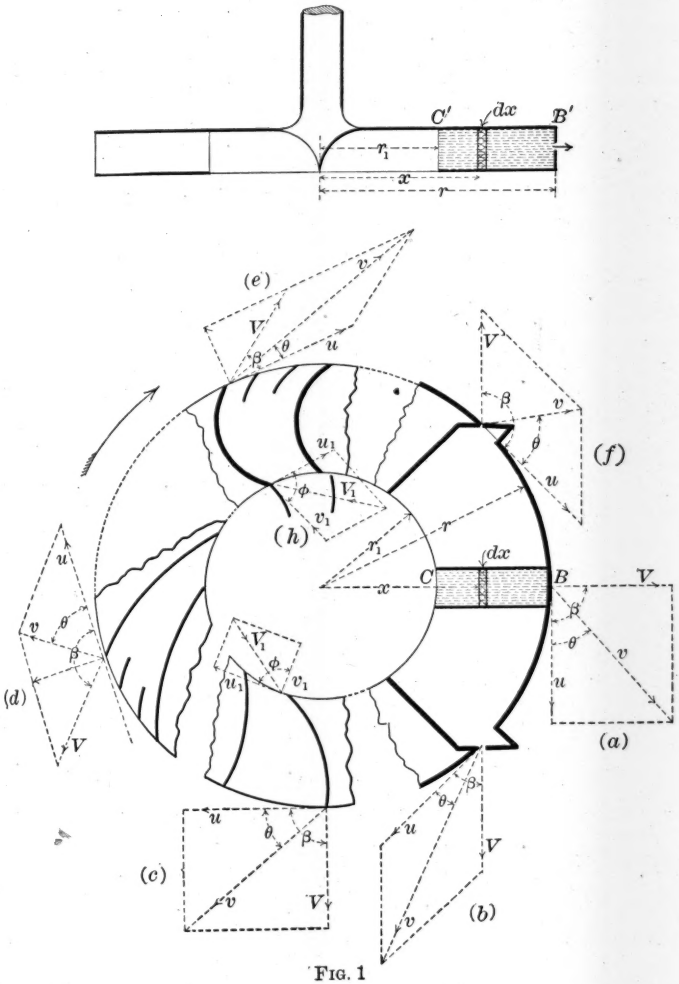

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FIG. 1

β = Angle between V and u;

θ = Angle of discharge, relative to a tangent to wheel = angle between v and u;

ϕ = Angle between v_1 and $u_1 = 90°$, unless guide vanes be inserted;

h = Head, or lift, above pump;

H = Total effective lift;

k = Initial or entrance head, which is usually suction, or negative head;

p = Pressure head in space between wheel and casing = pressure head in water at instant after escape from wheel.

"Foot-pound-second" units will be used.

Purely Centrifugal Effects.—In this preliminary study the reader should disincumber his mind of the more complicated conditions under which centrifugal pumps actually work. We are now seeking the relations between velocities unaffected by change of pressure. Later, the suction and discharge pressures will be introduced.

Consider a prism of liquid, of unit area and extending between the limits r_1 and r, in a revolving wheel without outlet, as $C\ B$, Fig. 1. The centrifugal force of an elementary disk across this prism, of thickness, $d\,x$, and distance, x, from the axis of revolution, is

$$d\,f = \frac{W\,u_x^2\,d\,x}{g\,x}$$

where u_x is the velocity of revolution at the distance, x, from the center. Thus

$$u_x = \frac{x}{r}\,u,$$

therefore

$$d\,f = \frac{W\,u^2}{g\,r^2}\,x\,d\,x,$$

and the total centrifugal force, f, which is effective at the outer end of this prism, is the integral between the limits, $x = r$ and $x = r_1$.

Hence

$$f = \frac{W\,u^2}{2\,g\,r^2}\,(r^2 - r^2_1) = \frac{W}{2\,g}\,(u^2 - u^2_1), \text{ since } u_1 = \frac{r_1}{r}\,u.$$

This is the pressure on a unit area at the circumference of the wheel, and, evidently, it is independent of the form or cross-section of the arm, $C\ B$. Now, pressure divided by weight gives head. Hence the pressure head against the walls of the pump at the circumference

$$= \frac{u^2 - u^2_1}{2\,g}$$

Now, if an orifice be opened at the circumference, in any direction whatever, and the pressure outside be the same as at the entrance, the velocity of the discharge, relative to the revolving walls of the pump, will be

$$\sqrt{2\,g \times \text{head}},$$

or

$$V^2 = u^2 - u^2_1;$$

but when there is a discharge there must be an initial velocity, V_1, at the entrance, and this must be considered in the final head within the wheel. Thus, the total head at B will now be

$$\frac{V^2_1}{2\,g} + \frac{u^2 - u^2_1}{2\,g};$$

and the velocity of the discharge, relative to the revolving parts, will have the relation

$$V^2 = V^2_1 + u^2 - u^2_1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (I)$$

Suppose, now, that $C\,B$ is a radial frictionless tube, open at both ends, and that a particle starts from a state, V_1, relative to the tube, and moves out, without change of pressure, from radius r_1 to r, in obedience to the law of centrifugal force (or acceleration). Its radial acceleration, when distant x from the center, is, by well-known laws of mechanics:

$$\text{Acceleration} = \frac{u^2_x}{x} = \frac{d\,V_x}{d\,t}.$$

Also,

$$V_x = \frac{d\,x}{d\,t}.$$

Therefore,

$$V_x\,d\,V_x = \frac{u^2_x\,d\,x}{x};$$

but, as before,

$$u_x = \frac{x}{r}\,u,$$

(sub x indicating the conditions at the distance, x, from the center.) Therefore,

$$V_x\,d\,V_x = \frac{u^2}{r^2}\,x\,d\,x.$$

Integrating between the limits, V and V_1, on one side, and, r and r_1, on the other, we get

$$V^2 - V^2_1 = u^2 - u^2_1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (I)$$

Combined Centrifugal and Impulsive Effects.—In Equation I the reader will probably recognize the well-established relation of veloci-

ties in impulse water-wheels. It is demonstrated in Church's "Notes on Hydraulic Motors," and is there shown to apply to either pumps or motors as long as they act impulsively. It has not been recognized, however, as being applicable to purely centrifugal effects acting statically, as demonstrated on page 171.

The demonstrations of Equation *I* evidently apply to any liquid, and to any gas in which the density may consistently be assumed as unchanged throughout the operations. (See page 213.)

It has now been made apparent that Equation *I* will hold, whether or not the section of the channel between *C* and *B* is constant, and without regard to the direction of the discharge; that is, the discharge may be outward, inward, backward, forward, upward or downward; that it is true whether the accumulation of centrifugal effect appears as pressure alone or as velocity alone, or partly in both; and that it is true for purely impulsive effects and for combined centrifugal and impulsive effects. The formula, therefore, is much more general than has been supposed heretofore.

Absolute Velocity of Discharge.—Having now a formula for computing the velocity of the escape of water from an opening in the perimeter of a wheel, relative to the wheel, its absolute velocity can be obtained from the triangle fixed by u, V, and the included angle, β, Fig. 1.

Thus, evidently, in general,

$$v^2 = V^2 + u^2 + 2\,u\,V\cos.\,\beta \ldots\ldots\ldots\ldots\ldots (II)$$

where β is the angle between V and the forward direction of u. Note that when β exceeds 90°, its cosine is minus. In particular, when $\beta = 90°$, or the vanes are radial at the outer end and $p = p_1$, then

$$v^2 = V^2 + u^2 \ldots\ldots\ldots\ldots\ldots\ldots\ldots (III)$$

It is well, here, to call attention to the effect that the angle, β, has on v. Notice that u and V are constant in all the several diagrams of Fig. 1, yet v is very different in the several cases. Now, if the reader will remember that the head varies with the square of the velocity he will see that the theoretically possible head, or lift, in Case (*e*) is about six times as great as in Case (*d*). (See also page 181.)

Pressure Head in Discharge and Entrance.—The foregoing formulas for V and v are derived on the assumption that the water is discharged into a medium in which the pressure is the same as in that from which the water entered the wheel, as, for instance, atmospheric pressure

within the entrance and outside of the wheel. This, evidently, is not a condition under which centrifugal pumps work ordinarily. Usually, there is "suction" inside the wheel, and, it is generally (erroneously) supposed, a pressure nearly equivalent to the remaining lift, opposing the discharge. Some relations involving this phase of the subject are as follows. See Fig. 2.

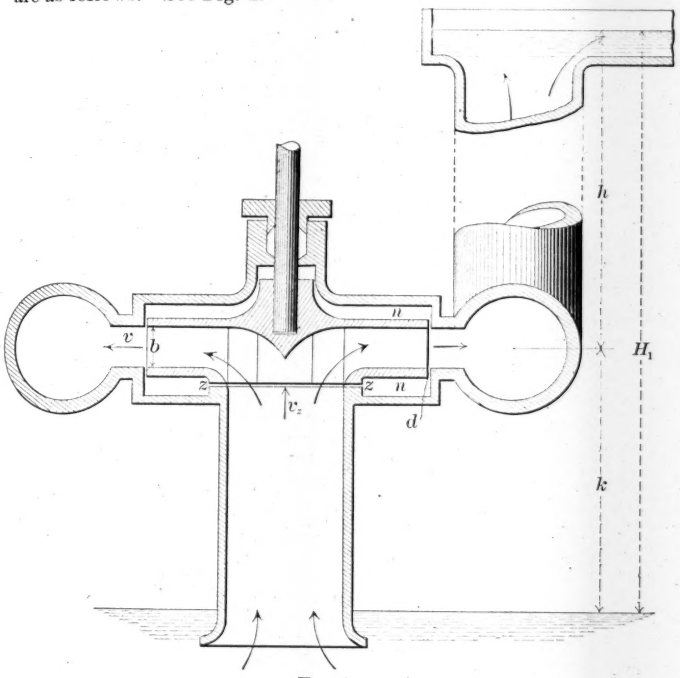


FIG. 2.

Let p_1 = pressure head at entrance of vanes, where velocity is v_1. Then

$$k = p_1 + \frac{v_1^2}{2g}.$$

Let p = pressure head in water immediately after escaping from wheel, where velocity is v, that is, at the joint, d, Fig. 2.

Then remember that whatever head, H, or lift, the wheel gives to the water, it is accomplished while passing out between the vanes.

Remember, also, that the total head in the water immediately after escaping from the wheel (that is at the joint, d) must equal the remaining lift, h.

Heads above the wheel are plus, those below are minus.

Then,

$$k + H = \frac{v^2}{2g} + p = h,$$

by the law of conservation of energy, or

$$\frac{v^2}{2g} = h - p. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (IV)$$

and

$$H = \frac{v^2}{2g} + p - k. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (V)$$

If, in Equation IV, p = zero, or atmospheric pressure, then $\frac{v^2}{2g} = h$; and if $p = k$, in quantity and in sign, then $\frac{v^2}{2g} = H$; which are evidently correct.

Note that p cannot equal h, as is usually supposed, for then v would be zero, and there would be no discharge.

Note that the signs in Equations IV and V are general, but that p and k may be, in themselves, plus or minus. Care must be taken to observe this, or confusion will result.

If possible, in practice, p_1 should equal p, for then there will be no tendency to leak at the joint, z.

Notice that the pressure, p, at the joint, d, will extend throughout the spaces, n, n, between the wheel and the casing.

The question now turns on this unknown quantity, p. That it is a factor we cannot ignore, is evident, for it affects the volume of discharge, and it renders useless the formulas thus far given, unless it can be determined and controlled.

The processes by which Equation I is reached are not applicable unless $p = p_1$, and has the same sign, but, fortunately, the method used on page 170 can be extended to meet the present needs.

When there are no guide vanes (and this will be the common condition), the absolute velocity of approach, v_1, will be radial, as at C, Fig. 1. In that case $V^2_1 = v^2_1 + u^2_1$.

Inserting this in Equation I, we get

$$V^2 = v^2_1 + u^2.$$

Hence the head, relative to the wheel, within itself, at the perimeter,

due to centrifugal effects, independent of pressures before entering, is $\frac{v_1^2}{g} + \frac{u^2}{2g}$. Now, if the total head at the entrance is k, the pressure head there will be $k - \frac{v_1^2}{2g}$, and the total head within the wheel at the perimeter is

$$k - \frac{v_1^2}{2g} + \frac{v_1^2}{2g} + \frac{u^2}{2g} = k + \frac{u^2}{2g}.$$

Then, if the pressure outside is p, the velocity of escape through an orifice at the perimeter in any direction is

$$V^2 = 2g\left(k + \frac{u^2}{2g} - p\right) = u^2 + 2g(k - p) \ldots\ldots (VI)$$

Note that k and p may be either positive or negative in themselves, and in numerical calculations the proper sign must go with them. For instance, if they are both negative (the common condition), their signs will be reversed in Equations *IV*, *V* and *VI*.

In Equation *VI* we have a formula for V (the relative velocity) that can be used with confidence, provided we know p. More on this subject will be found on pages 185 to 188.

From Equations *II*, *V* and *VI* we can get

$$gH = u^2 + u\,V\cos.\,\beta \ldots\ldots\ldots\ldots\ldots (VII)$$

or,

$$gH = u^2, \text{ when } \beta = 90°.$$

Which shows that H is independent of k and p. It is interesting to note that $\frac{u^2}{2g}$ is usually stated to be the maximum theoretic lift, regardless of the angle, β (see page 178).

Independent Derivation of Chief Formulas.—The following independent demonstrations are interesting, and will strengthen our confidence in the conclusions otherwise reached.

If, in Fig. 1, the vanes take up the water from a state of absolute rest and discharge it at the perimeter, the whole work done by the wheel can be expressed in terms of the reaction of the escaping water on the wheel. This reaction, by well-known laws of hydro-mechanics, is $\frac{Wv}{g}$, and the component opposed to the motion is $\frac{Wv}{g}\cos.\,\theta$. Then the work done by the wheel in overcoming this reaction is $\frac{Wv}{g}\cos.\,\theta\ u$. Then, assuming the pressure opposing the discharge

to be the same as before admission, the energy in the water after escape is $\frac{W v^2}{2 g}$.

Hence,

$$\frac{W v^2}{2 g} = \frac{W}{g} u v \cos. \theta.$$

or,

$$v = 2 u \cos. \theta.$$

This being true, a glance at the trigonometrical relations in Fig. 1 will show that $v = u$, and $\theta = \frac{\beta}{2}$.

The assumption, that the water is at rest until caught up by the vanes, is not correct when the water is passing through. To get an equation meeting this condition we have only to remember that the static pressures inside and outside a wheel, though different, cannot directly aid or resist its revolution, and that, in pumps, we may assume the velocity of approach, v_1, to be caused by the pump, and, therefore, its effect on the wheel is not to be considered, it being only a preliminary incident to the final discharge. Hence, the work against the reaction of the discharged water will always be the total work done by the wheel. Then, by the law of the conservation of energy, the following equation must hold:

$$W k + \frac{W v}{g} u \cos. \theta = \frac{W v^2}{2 g} + p W = W h \ldots\ldots (a)$$

Hence,

$$2 u v \cos. \theta = v^2 + 2 g (p - k).$$

Now, insert the value of $2 u v \cos. \theta$, from the trigonometrical relation, $V^2 = u^2 + v^2 - 2 u v \cos. \theta$, and the equation reduces to

$$V^2 = u^2 + 2 g (k - p) \ldots\ldots\ldots\ldots\ldots (VI)$$

which is identical with Equation *VI*, on page 175.

From Equation a we have $H = \frac{v^2}{2 g} + (p - k)$, since $H = h - k$ (see Fig. 2), and, by trigonometry, $v^2 = V^2 + u^2 + 2 u V \cos. \beta$. Now substitute this and the value of V^2 from Equation *VI* and we get

$$H = \frac{u^2 + u V \cos. \beta}{g} \ldots\ldots\ldots\ldots\ldots (VII)$$

as on page 175.

It is remarkable, and to the writer quite unexpected, that Equations *VI* and *VII* show no factor involving the radial depths of the propellers, that is, the relation between r and r_1; but careful considera-

tion of all the conditions involved will make it clear that such should be the case. On page 171 the formula, $V^2 = u^2 - u^2_1$, may be written $V^2 = u^2\left(1 - \frac{r^2_1}{r^2}\right)$, since $u_1 = \frac{r_1}{r}\, u$. Here, then, we find the relation between r_1 and r involved, and the analysis there given shows that it must be; but, in that case, there is, to that point, assumed no movement of water through the pump. When water flows into the pump it must be caught up by the propellers, and the velocity suddenly changed from v_1 to $\sqrt{v^2_1 + u^2_1}$. Thus it is seen that the pump must do work on the water in the act of picking it up; and this is independent of the static head (considered relatively to the wheel) imparted between the limits r_1 and r. Without adhering to a strictly correct use of mechanical terms, perhaps, we may say that with a fixed r and u the head imparted by the static effect of centrifugal force decreases as r_1 increases, while the work done by impact (or reaction) in taking up the inflowing water increases with r_1; the sum of the effects in imparting energy to the departing water being constant when r_1 only varies.

Volume of Discharge.—Referring to Fig. 1, it is seen that the radial component of V is $V \sin. \beta = v \sin. \theta$, hence the volume of discharge is

$$Q = a\, V \sin. \beta = 2\, \pi\, r\, b \sin. \beta\, V. \ldots\ldots\ldots (VIII)$$

assuming a continuous opening or slot, of width, b, around the perimeter.

Evidently, the entrance to the vanes or propellers must be proportioned to comply with

$$Q = 2\, \pi\, r_1\, b_1\, v_1 \sin. \phi \ldots\ldots\ldots\ldots\ldots (IX)$$

$v_1 \sin. \phi$ is the radial component of the velocity of the water as it enters between the vanes. This will usually be assumed to be between 10 and 20 ft. Unless guide vanes are inserted, as at (*h*), Fig. 1, and (*b*), Fig. 15, the angle, ϕ, will be 90°, and Equation *IX* will become

$$Q = 2\, \pi\, r_1\, b_1\, v_1 \ldots\ldots\ldots\ldots\ldots (IXa)$$

Present Practice, in the Light of the Preceding.—The foregoing discussion, with the resulting equations, will give light on matters not heretofore generally understood (judging from correspondence, the contents of trade circulars, and the proportions of pumps as built). For instance, it is a common statement that the theoretic lift depends only on the velocity, u, of the outer limit of the propellers, and that the theoretic lift is $\frac{u^2}{2\, g}$. Equation *VI* shows this to be doubly in error.

True, after a wheel is built, u is the only variable affecting the lift, but, in the intelligent design of a pump, the angle, β, is an important factor.

Some writers, whose opinions are apt to be accepted without question, state that when the propellers are curved backward, as in Fig. 5, there is, in addition to purely centrifugal effects, an outward "shove," or impulse, given to the water, as it escapes, by the tips of the propellers. In making this claim, they probably forget that the water is revolving with the propellers before it reaches the end. Evidently, when the water is moving with the propeller no impulsive action can occur between them. If the outer few inches of the propellers came in contact with still water, the effect claimed would occur, and the result would be an impulse pump.

The writer can find no theoretic reason for curving the propellers backward, but rather the reverse. (See page 172.)

In several published articles which the writer has examined, prominence is given to the test for the effect of curvature of the vanes, the test being made by running the pump without discharge and measuring the resulting pressure or head by a gauge or stand-pipe, thus getting the relation between the peripheral speed and the head for differently curved vanes.* In the light of the discussion on page 170, this scheme is fallacious, for, as the propellers, of whatever form, revolve, friction will impart the motion of the liquid to that within the inner limits of the vanes, and in a few moments all the liquid, from the center out, will be revolving with the vanes. Then, as shown previously, the pressure head at the perimeter will be

$$\frac{u^2}{2g} - \frac{u_1^2}{2g} = \frac{u^2}{2g},$$

since u_1 is zero. The curve of the vanes would have nothing to do with it. Where a discharge occurs, the curvature affects the head, but not otherwise. That varying results were actually gotten, must have been due to some circulation through the open joints, d and z, around the discharge and intake.

Unfortunately, the above-mentioned experiments, as misinterpreted, have apparently verified the very common supposition that the theoretic lift of a centrifugal pump is $\frac{u^2}{2g}$. There seems to be almost universal confusion and error concerning this, and universal neglect to consider correctly the pressure at the open joint, d. Correct

* *Engineering News*, August 9th, 1900; and Richards' "Centrifugal Pumps," p. 27.

equations bearing on these points—Equations *IV* to *VII*—have been given, but, to clear up the confusion, let us recall the fundamental principle in hydraulics, that "total head equals velocity head plus pressure head," and apply it to the water passing the joint, d, after it has left the propellers. Then, using the notation elsewhere applied, the head in the water passing d is $\frac{v^2}{2g} + p$, and, neglecting friction, this must equal the remaining lift, h, or, $h = \frac{v^2}{2g} + p$.

Now, v is greater than u, except when β exceeds 90 degrees. Evidently, then, we cannot consistently assume that the lift above the pump is $\frac{u^2}{2g}$, and that the pressure head, p, at the discharge is that due to the same lift, for that would be assuming, by the equation above, that $h = 2h$ (or more when β is less than 90°).

Suppose the pump is revolving without discharge, then the equation becomes $h = \frac{u^2}{2g} + p$, but, on measuring the head in a stand-pipe we would find it $= \frac{u^2}{2g}$, only. Still, the equation is correct. The explanation is that the velocity head, $\frac{u^2}{2g}$, in the water is not transformed into pressure head. When a discharge occurs the water passing out must lose its velocity, and, theoretically, should gain in pressure correspondingly. This subject is discussed further on page 188.

If, in Equation *V*, *viz.*, $k + H = \frac{v^2}{2g} + p = h$, we substitute the value of H from Equation *VII*, we have

$$p = \frac{u^2 + u\,V\cos.\,\beta}{g} - \frac{v^2}{2g} + k.$$

Now, in centrifugal pumps, as usually constructed and operated, the absolute velocity, v, is but little greater than the peripheral velocity, u. Hence, for the purpose of illustration, we will assume $u = v$. Then the above equation becomes:

$$p = \frac{u^2}{2g} + \frac{u\,V\cos.\,\beta}{g} + k.$$

To show by example what p would be in an ordinary pump, assume

$u = 50$, $V = 5$, $\beta = 120$ (or cos. $\beta = -0.5$), and $k = -10$.

Then, by Equation *VII*, $H = 74$, and therefore $h = 64$, and, by the above equation, $p = 25$.

One cause which adds to the confusion in this matter is that the efficiency of such pumps is not far from one-half, thus making the actual lift attained about one-half the theoretic lift, or

$$H = \frac{u^2 + u\ V \cos.\ \beta}{2\,g},$$

instead of

$$H = \frac{u^2 + u\ V \cos.\ \beta}{g}.$$

It is very important that the pressure, p, in any test, be measured, for by its aid we can interpret results much more intelligently than has been done in the past. For instance: The lift, h, above the pump should equal $\frac{v^2}{2\,g} + p$, and the deficiency shows what is lost after the water passes the joint, d. Thus we may determine where the greatest loss occurs.

Balancing.—The much-discussed subject of balancing centrifugal pumps should be studied in the light of the preceding remarks.*

The assumption that the pressure head under the propeller disk is that due to the lift above the wheel is certainly wrong. The problem is different in every design, and, in any design, the proportions affect the results.

The following special case is interesting: The wheel, $A\ A$, Fig. 3, revolves over the fixed plane, $B\ B$, with peripheral velocity, u. The space, $C\ C$, is filled with water which revolves with A, and, by virtue of its centrifugal force, exerts a pressure downward on B and upward on A. What is the force tending to lift A?

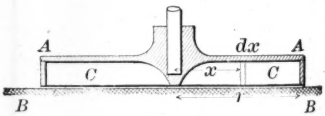


FIG. 3.

The head, at the distance, x, from the axis, is $\frac{u_x^2}{2\,g}$, where $u_x = \frac{x}{r}\,u$. The upward pressure on a ring, of radius, x, and radial width, $d\,x$, is

$$d\,p = 2\,\pi\,x\,d\,x \times W \frac{u^2\,x^2}{2\,g\,r^2} = \pi\,\frac{W\,u^2}{g\,r^2}\,x^3\,d\,x.$$

Integrating between $x = r$ and $x = 0$, we get

$$P = \frac{\pi\,r^2}{2} \times W \frac{u^2}{2\,g}$$

which is half the pressure on a circle, of radius r, due to a head of $\frac{u^2}{2\,g}$. This uplift is doubled in some designs proposed later herein.

* *Engineering News*, Vol. 38, p. 75.

Form and Depth of Vanes.—According to the mathematical analysis on page 174, the ratio, $\frac{r_1}{r}$, does not affect the head under which the pump can work. Notwithstanding that, the ratio should be selected with care, in order that the pump may be otherwise proportioned conveniently and efficiently. In the light of the preceding, there are no grounds for making the outer radius, r, two or three times the inner radius, r_1, as seems to be the common practice.

Some builders state that every particle of water should pass around with the wheel at least once, and most of them accept that idea (judging by the proportions of their pumps). This is probably due to some notion that full centrifugal effect is not produced until the particle has made one revolution around the axis. The seriousness of this error in pumps, as heretofore designed, lies in the fact that, in proportioning them accordingly, the area of the surface over which the water moves at a high velocity is made much greater than necessary. While there are lacking experimental data on the friction of water passing over smooth surfaces at such velocities as occur in pumps, such facts as are known point to very serious loss due to this cause.

It is the writer's opinion that the less the radial depth of the propellers the better; provided that every particle of water be brought to one condition, as to pressure and velocity, before escaping from the wheel; and that the inner tips of the propellers (vanes) be set to part the approaching water without shock. It may be noted here that if the vanes be made short it becomes impracticable to make β much less than 90°, except in the special design shown in Fig. 11.

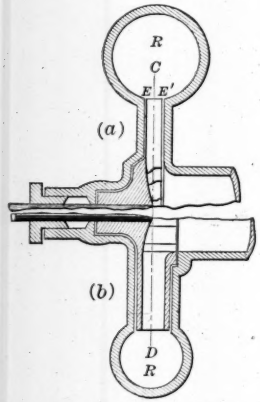
It is evident that the inner terminus of the vane is immaterial, theoretically, provided it has the proper forward inclination, as specified above. The simplest construction is that shown in Fig. 11, where the vanes are sheared off at their inner limits parallel to the axis and reaching only to the periphery of the circular inlet. If they extend to the center, as in Fig. 6, the forward inclination of the edges should vary from nothing at the center to the maximum at the outer limit of the inlet.

Influence of Discharge Passages.—Thus far, this study has been confined to the means of propulsion and its effects on the fluid. The influence of the passage which conducts the liquid from the perimeter of the propelling wheel to the final straight discharge pipe will now

be studied. Here, conclusions even more interesting than in what has preceded will be found.

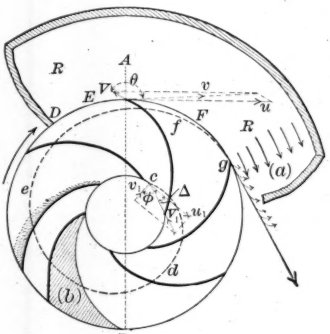
We can probably best lead up to, and understand, this part of the subject by following out the action in a pump of such proportions as are known to be common. Then modifications can be suggested.

The following data are taken from a published test of a pump made by one of the best manufacturers (unimportant fractions being omitted) (see Figs. 4 and 5):



SECTION *A-B* OF FIG. 5.

FIG. 4.



SECTION *C-D* OF FIG. 4

FIG. 5.

r = 4 ins.;

r_1 = 12 ins.;

β = 153° ; therefore, cos. β = — 0.89;

u = 57 ft. per second;

Q = 3 cu. ft. per second;

k = — 21 ft.;

H = 35 ft., as measured.

The width of the propellers is not given, but it will be assumed to be 2 ins., which will make the area of the circular inlet just equal to the area of the inlet between the propellers, that is, the water has a constant velocity of 8.6 ft. per second as it enters the wheel and passes in between the propellers.

The angle, Δ, between V_1 and u_1 is not given, but it should be 24° 15′ in order that the water may glide in without shock or break

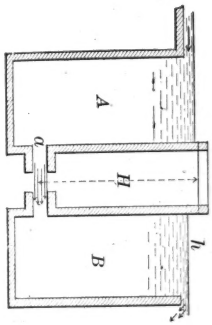


Fig. 8

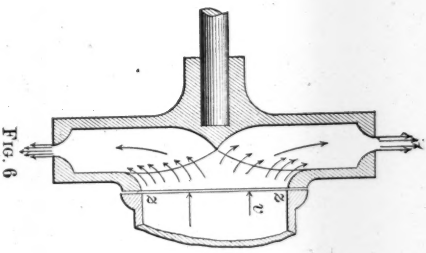


Fig. 6

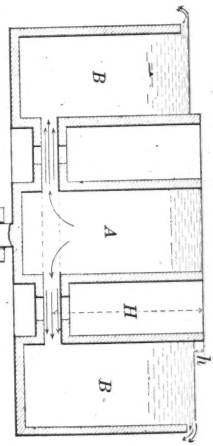


Fig. 9

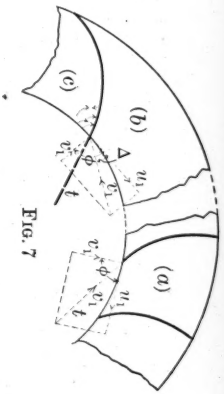


Fig. 7

(see Fig. 7). In such wheels it is common to make the propellers of constant width. Assuming this to be so, the outlet area will be three times as great as the inlet, and the radial component of the discharge velocity, therefore, will be one-third of 8.6, or nearly 2.9. Whence, from Fig. 1, evidently

$$V = \frac{2.9}{\text{sin. } \beta} = 6.8.$$

Then, by Equation *VII*, remembering that cos. β is minus, we get $H = 90$. As the recorded total lift was 35 ft., this would show an efficiency of 40 per cent. This does not agree with the report. However, some of the proportions have been assumed, and a glance at Fig. 5 will show the triangle involving V, u and v in a form unfavorable to accuracy.

From Equation *II*, we find $v = 51$. Then, by Equation *IV*, we have $h - p = 41$, and since $H = h - k$, and $k = -21$, h must be 69. This makes $p = 28$.

The reader must bear in mind that these are the theoretically perfect results. We know, from the results of tests, that a large part of the energy is lost somewhere.

Causes of Loss of Energy.—Now, let us discover the chief causes of loss of energy. When understood, we may then find means of avoiding, or, at least, reducing them.

(*a*)—The inner tips of the vanes should point opposite to the relative motion of the incoming water, or tan. $\phi = \frac{v_1}{u_1}$, where no guide vanes are inserted (and this will, doubtless, be the common practice), Fig. 7 (*a*). To fulfill this condition, it is evident that the discharge must vary directly with the velocity of revolution. Hair-splitting precision in this matter is not essential nor practicable, but a gross neglect will result in shock at the point of the vane and eddies just behind it. In extreme cases of neglect of this point a vacuum may occur at about (*c*), Fig. 7, especially where the suction, k, is large.

(*b*)—From the example above, we note that the radial velocity at the entrance between the vanes is 8.6 and at the discharge 2.9, the average being 5.7, while the outer tip of the vane has a velocity of 57.0. Under these conditions, the wheel makes a little more than one revolution while a particle of water passes from the inner to the outer

limit of the vanes. During this time the water slips against the fixed sides of the casing, a distance of more than 6 ft., with a (relative) velocity varying between 19 and 57 ft. per second. This is true whether the propellers be of the open type, Fig. 4 (*a*), or the inclosed type, Fig. 4 (*b*); for, in the latter, the water surrounding the runner must either slip, relative to the runner, or relative to the fixed casing, or both. This is probably one of the chief causes of loss.

(*c*)—In the example taken we found $u = 57$. Now, if we knew all the dimensions we could compute the mean velocity in the volute, *R*, Fig. 5. It should not, and does not in practice, exceed 10 ft.

Then we would have, along the surface, *E E'*, or *D E F*, one body of water moving against another with a relative velocity of 47 ft. or more. If the wheel is of the "hollow-arm" type, that is, part of the wheel is solid, as at (*b*), Fig. 5, the last two causes will be even more serious. Here must be another important cause of loss. It is sometimes called the "drag" at the tip of the propellers. See Fig. 5 (*a*).

(*d*)—Lastly: When the section of a flowing stream is suddenly enlarged, as at *E E'*, some loss occurs.*

We have thus followed the water through the wheel, have noted the causes of loss of work, and have, to some extent, indicated the degree of importance of each. They are not of a nature that can be analyzed mathematically, but are none the less real. It should be mentioned, perhaps, that these losses depend on the construction (or design) of the wheel, its speed, discharge, etc.

The remedy for loss (*a*) is apparent. That under (*b*) can be reduced by decreasing the radial length of the vanes and by curving the vanes forward, thereby reducing the necessary speed. See page 177.

Losses (*d*) and (*c*) can be avoided by means now to be described.

The radical departure here proposed, in the construction of centrifugal pumps, depends chiefly on two principles, both well known, but neither, to the writer's knowledge, having been applied in this connection.†

The discharge from the vessel, *A*, Fig. 8, will enter *B* as long as the drop, *h*, is equal to the losses by friction, etc.; and the velocity of this discharge is dependent on the head *H*. Thus, without increasing the area of the orifice, *a*, or the effective head, *h*, we can increase the

*Cyclop. Brit., Vol. 12, Art. 32, p. 469.
†Cyclop. Brit., Vol. 12, Art. 28, p. 467, Fig. 36, reproduced here in Fig. 8.

discharge indefinitely by increasing H. Now, suppose the vessel, B, to be continuous around A, and that both orifices are continuous annular slots, as in Fig. 9. Under this condition, the vessel, A, can be revolved about its vertical axis without interfering with the discharge or its reception in B. This hints at the application of the principle to increase the flow from centrifugal pumps. But a difficulty yet stands in the way: If the head in A is due to centrifugal effect, and that in B is the resulting lift, there will be closely limited relations between the rate of revolution, lift, etc., which, if changed, will result either in water not entering B (when the speed is too low), or in air entering with the water (when the speed is too high). To control this better, and for other reasons, apparent later, we will apply the second principle, which is that of the Sprengel Pump, or aspirator. See Fig. 10. The outwardly-flaring nozzle placed on the vessel, B, will cause a suction at its least section when the energy of the discharge from A is greater than necessary to lift the water in B, or against the required head.

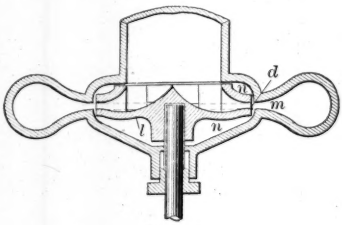


FIG. 10.

This suction will tend to exhaust the air from the space, n, surrounding the wheel, l. As the exhaustion increases (or the pressure head, p, drops) the discharge will increase until a state of equilibrium is reached. Then the wheel will turn in partial vacuum instead of in water, there being no leakage of air inward nor of water outward, though a clear joint is left between the wheel and the receiving nozzle, m. The high velocity of discharge will give an absolute direction of discharge a little less than 45° outward from a tangent when $\beta = 90$ degrees. The flaring receiving nozzle, m, will reduce this velocity gradually without serious loss of energy. Notice that all "thrashing" of water by impellers, and all "drag" of rapidly moving water against that which is comparatively still, are eliminated.

Here, then, we have a means of avoiding entirely the losses (b), (c) and (d). In their place will occur whatever loss is unavoidable in the nozzle, m. The writer knows, from tests made by himself, that this can be reduced to 15 per cent. See page 205. In such a wheel the area of the passage through the wheel should be as large as

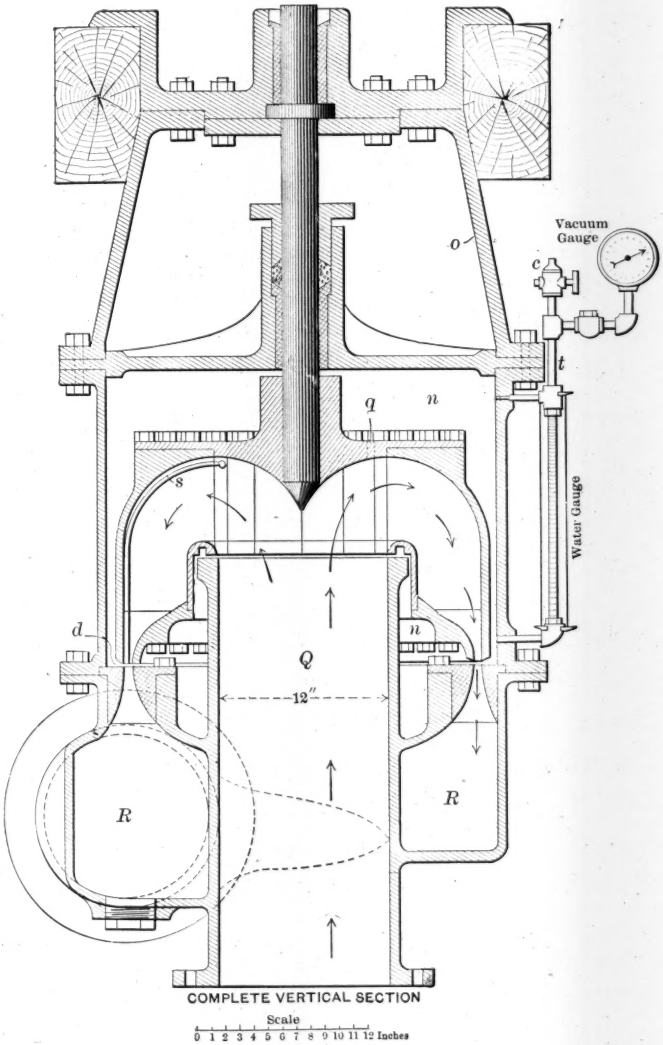

Vacuum Gauge
o
c
t
q
n
s
Water Gauge
d
n
Q
12″
R
R
COMPLETE VERTICAL SECTION
Scale
0 1 2 3 4 5 6 7 8 9 10 11 12 Inches

FIG. 11.

convenient, to reduce velocity, and thereby friction loss, inside the wheel, and to give all particles time to come to one condition of pressure and velocity. The contraction for the outlet should be a sharp curve near the periphery. Note that the only sliding friction of the water against the wheel occurs in the radial movement of the water relative to the wheel. It matters not how many times the water goes around within the wheel.

The suction or minus head, p, in the space, n, probably cannot be specified until some further experimenting is done. (In some experiments of the writer with straight nozzles it was nearly 20 ins. of mercury.)

Fig. 11 presents the writer's idea of a construction well suited to secure the conditions desired, as pointed out in the preceding paragraphs.

The design shown in Fig. 11 has the further advantage that the reaction of the discharge can support the weight of the wheel partly or entirely. It has also the advantage of compactness, and the discharging and receiving nozzles can be more easily kept opposite.

Starting.—On page 178 it is shown that when a centrifugal pump is revolving without discharging, the greatest pressure head it can produce is $\frac{u^2}{2g}$, while, by Equation *VII*, the lift, when in operation, is found to be $H = \frac{u^2 + u\,V \cos.\beta}{g}$. Now, since the writer recommends that cos. β be always positive, or zero, it is evident that a wheel cannot start a discharge, when under back pressure, from the maximum lift under which it can work after being started. This difficulty is overcome readily by putting a check-valve in the discharge, and between that and the pump a valved branch through which the discharge can be made temporarily under low head. Then, when the pump gets its normal speed and discharge, the starting valve, f, Fig. 12, can be closed gradually, and the flow will be directed into the main pipe.

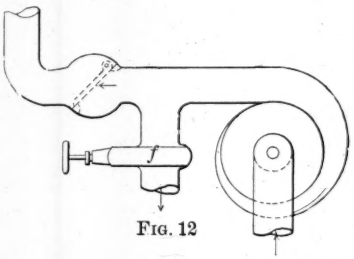


FIG. 12

In case the greater part of the total lift, H, is in suction, that is,

when k is negative and nearly equal to H, numerically, the branch pipe carrying the valve, f, should descend into the sump, or source from which the water is taken. That this is necessary will appear from an examination of Equations *VI* and *VII*.

$$V^2 = u^2 + 2\,g\,(k - p)$$

and

$$g\,H = u^2,\ (\beta \text{ assumed } 90^\circ).$$

From these we get

$$V^2 = g\,(H + 2\,k - 2\,p).$$

Now, if k is negative and greater than half of H, and p is zero or any positive quantity, the second member becomes negative, or V indeterminate. Evidently, when k is negative and nearly equal to H, p must also be negative and greater than half of H. The arrangement specified above will insure these conditions at starting. After starting, p can be controlled through the cock, c, Fig. 11.

In Fig. 11, a small tube, s, starts from within the inner limits of the vanes and extends out to the discharge. The necessity and duty of these tubes will be apparent from the following:

If a vessel containing water and air revolves rapidly, centrifugal force will separate the two (independently of gravity), the lighter air being forced to collect about the axis of revolution. This will occur should air by any means get into any of the centrifugal pumps proportioned as in Fig. 11 (where the outlet is very much contracted as compared with the area of the passage through the wheel). The above will not hold, however, in a tube of constant cross-section leading away from the axis of revolution and open at the outer end, as the tube, s. On the contrary, there will be, in such a tube, a suction throughout its length due to the accelerated outward velocity of whatever water or air may be in the tube. This suction, being greatest at the inner end of the tube, will take out all bubbles that would otherwise collect about the center of the wheel and interfere with the action.

A close analogy to the above principle may be found in the suction caused by water flowing down an open vertical tube.

If, in practice, the suction, p, in the space, n, is found greater than k, the same purpose could be accomplished by a small hole in the top of the wheel, as at q, Fig. 11.

Method of Proportioning.—The formulas necessary in designing are:

$$V^2 = u^2 + 2\,g\,(k - p) \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (VI)$$

$$H = \frac{u^2 + u\,V \cos.\,\beta}{g} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (VII)$$

$$Q = 2\,\pi\,r\,b\,V \sin.\,\beta \ldots\ldots\ldots\ldots\ldots\ldots (VIII)$$

$$Q = 2\,\pi\,r_1\,b_1\,v_1 \sin.\,\phi \ldots\ldots\ldots\ldots\ldots\ldots \quad (IX)$$

These must be applied intelligently in order to work out the dimensions of the principal parts of a pump. The fixed conditions will usually be H, Q and k; p can be assumed with practical accuracy after some experimenting. For reasons heretofore given (pages 175 and 184), it ought to be, and probably will be, nearly equal to k. Then, until experiments give more reliable information, we may assume $p = k$. For reasons of simplicity and economy of construction, it is probable that the outer tip of the propelling vanes will be made radial, that is, $\beta = 90$ degrees. For the same reason, it is probable that the guide vanes at the entrance will be omitted, that is, $\phi = 90$ degrees.

With these simplifications the several formulas become:

$$V^2 = u^2 + 2\,g\,(k - p) \ldots\ldots\ldots\ldots\ldots\ldots (VI)$$

$$H = \frac{u^2}{g} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (VIIa)$$

$$Q = 2\,\pi\,r\,b\,V \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (VIIIa)$$

$$Q = 2\,\pi\,r_1\,b_1\,v_1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots (IXa)$$

The working out of proper proportions will now seem simple, but a difficulty will be found, in that b will in many cases be embarrassingly small. This fact alone will prevent the application of such pumps where the lift is great and the quantity small. (Modifications to overcome this difficulty are described on page 202.)

The most rational order of proceeding will be to assume a velocity through the intake channel, v_2, and also the velocity of entrance to the vane, v_1. These may both be taken safely at 10 ft. per second. As soon as the velocity through the intake is fixed, r_1 becomes known, from the condition, $\pi\,r_1^2 \times$ velocity $= Q$, for a single intake; or $2\,\pi\,r_1^2 \times$ velocity $= Q$, for a double intake. We can then find b_1 from Equation *IX*. Next, assume $\frac{r_1}{r}$ between 0.50 and 0.75. This will fix r. u will be found from Equation *VII*, V (relative velocity), from Equation *VI*, and b from Equation *VIII*.

Before leaving the subject of centrifugal pumps, attention should be called to the advantage of making both the discharging and receiv-

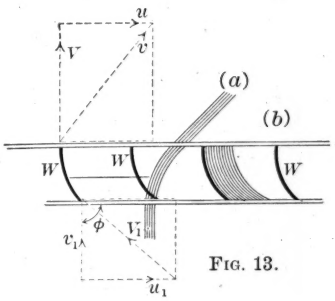


FIG. 13.

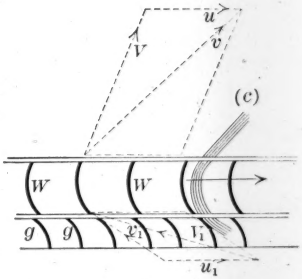


FIG. 14.

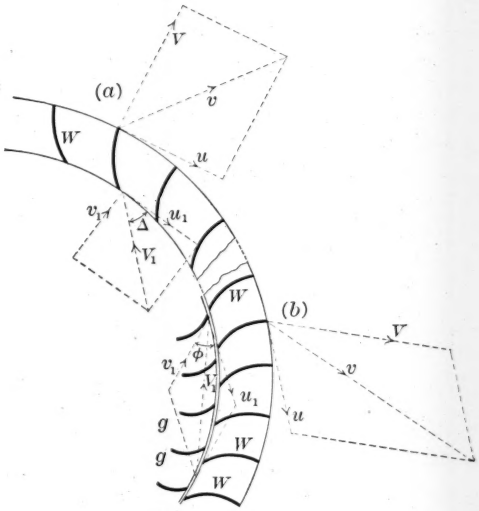


FIG. 15.

ing nozzles detachable. Then, with a few standard sizes of wheels and casings, nozzles can be attached, to meet any ordinary requirements, thus reducing the cost of manufacture; it is further desirable, on account of the liability of the nozzle to wear, especially if the water is at all gritty.

Impulse Pumps.

The name "impulse pump" is given by the writer to that class of rotary pumps in which the energy is imparted to the water by impulsive action. They must be placed in a class by themselves, for, though resembling centrifugal pumps in some points of construction and in their mathematical formulas, they are quite different in principle, and present another set of problems for solution. It is sometimes said that such pumps are the reverse of the impulse water-wheel. That is true, as to principle, but an impulse water-wheel turned backward would not make a pump.

To show that the action is independent of centrifugal force, suppose the vanes, *W W*, Figs. 13 and 14, to move forward in a straight line. Then there could be no centrifugal force, and yet the water would be caught up and thrown out at the upper edge with a velocity, *v*, as shown in the figures.

In practice, the vanes must be placed on the periphery of a wheel, Fig. 15, and, if the discharge is outward, it is apt to be mistaken for a centrifugal pump. In fact, in that case, there will unavoidably be some centrifugal effect; but, if proportioned to get the best results from impulsive effect, the centrifugal effect will be relatively small. If the vanes are set to move the water parallel to the axis, as in Fig. 16, there can be no centrifugal effects. The vanes may even be set to throw the water inward toward the axis.

Careful attention should be given to the following distinctive conditions in purely impulse pumps, viz.: There should be no change of pressure in the fluid as it passes out between the vanes, the energy imparted to the fluid appearing as increase of velocity alone. That this is true will appear on inspection of Fig. 13 (*b*), where the vane is only partly filled, the water being under atmospheric pressure throughout; yet as much energy will be imparted to the water in Case (*b*) as when the space between the vanes is filled. To meet this condition requires that the area of the passage between the vanes be proportioned carefully throughout. This is not necessary in centrifugal

pumps. It may be said here that purely impulse pumps are not likely to be attempted in practice. (See page 197.)

Formulas Relating to Impulse Pumps.—Consider first the general case of an outward-flow pump with guide vane, Fig. 15 (*b*). Then, using the symbols heretofore applied to centrifugal pumps, we have at the entrance

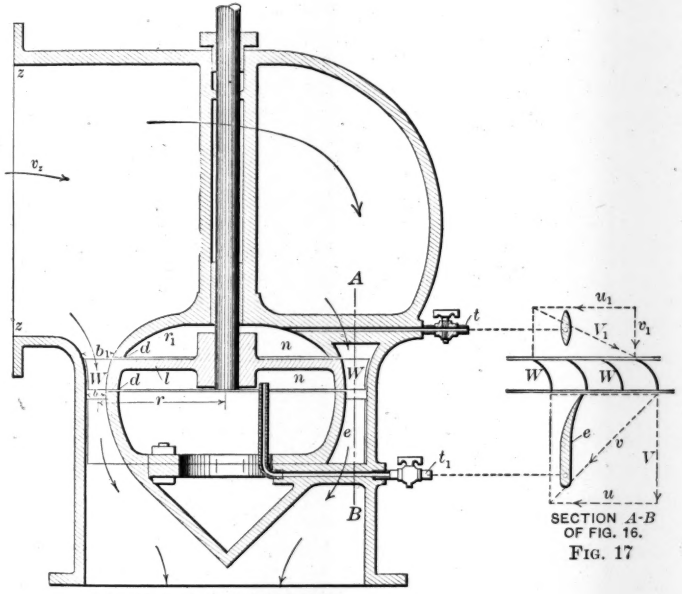
$$V_1^2 = v_1^2 + u_1^2 + 2\, u_1\, v_1 \cos.\ \phi \ldots\ldots\ldots\ldots\ (X)$$

Equation *I* applies to this case. Hence

$$V^2 = V_1^2 + u^2 - u_1^2 \ldots\ldots\ldots\ldots\ (I)$$

and at the discharge we have, as in centrifugal pumps,

$$v^2 = V^2 + u^2 + 2\, u\, V \cos.\ \beta \ldots\ldots\ldots\ldots\ (II)$$



PARALLEL-FLOW IMPULSE PUMP.
FIG. 16

SECTION *A-B* OF FIG. 16.
FIG. 17

These formulas are general, from a theoretic point of view. They show that to get the greatest lift, $\frac{v^2}{2\,g}$, with a fixed u and v_1, the angles, ϕ and β, should be as small as possible; but, from a more practical point of view, there are sharp limitations to this. Notice that the

length of the vanes is not involved in the foregoing formulas. Remember, also, that to reduce friction losses, the surface over which the water passes at a high velocity should be the least possible, consistent with other requirements. For this reason it will probably be found best to make the vanes short, even at the sacrifice of some of the curvature. Furthermore, the construction will be simplified and cheapened by omitting the guide vanes, g, g. Then ϕ becomes 90 degrees. For similar reasons, β will usually be 90 degrees. Under these conditions, substituting from Equations X and I, since cos. ϕ and cos. β are both zero, Equation II becomes

$$v^2 = 2\,u^2 + v_1^2 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (XI)$$

The same reasoning and the same formulas will apply where the flow is parallel to the axis, as in Fig. 16. In this case, $u_1 = u$, and therefore $V_1 = V$. This is also evident on inspection.

By the law of hydraulics, that velocity head plus pressure head = total head:

$$p + \frac{v_1^2}{2\,g} = k \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (XII)$$

Notice that p reaches the entrance vanes, for there is no change of pressure; and, since the head, H, must be imparted to the water while it passes between the vanes, we have, by the law of conservation of energy, neglecting the velocity in the discharge main,

$$k + H = \frac{v^2}{2\,g} + p = h;$$

then, taking the equivalent of $k - p$ from Equation XII,

$$H = \frac{v^2}{2\,g} - (k - p) = \frac{v^2 - v_1^2}{2\,g} \ldots\ldots\ldots\ldots\ldots (XIII)$$

In this case k can never equal p, for, if it did, v_1 would be zero, and no work would be done.

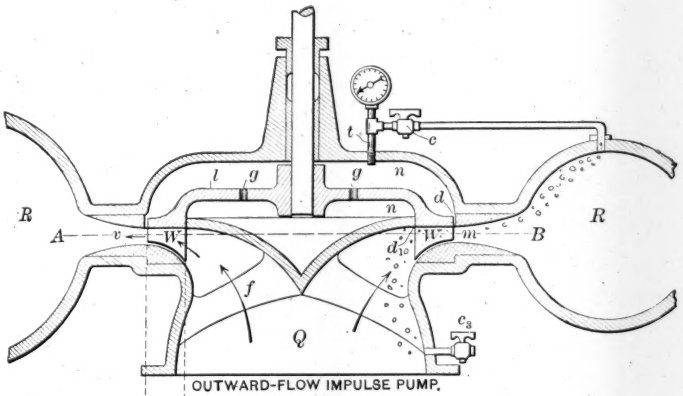
Remember that when p and k represent suction they are in themselves minus, independently of the signs of the equation.

If in Equation $XIII$, which is general, we substitute the special value of v^2 from Equation XI we get

$$w^2 = g\,H \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (XIV)$$

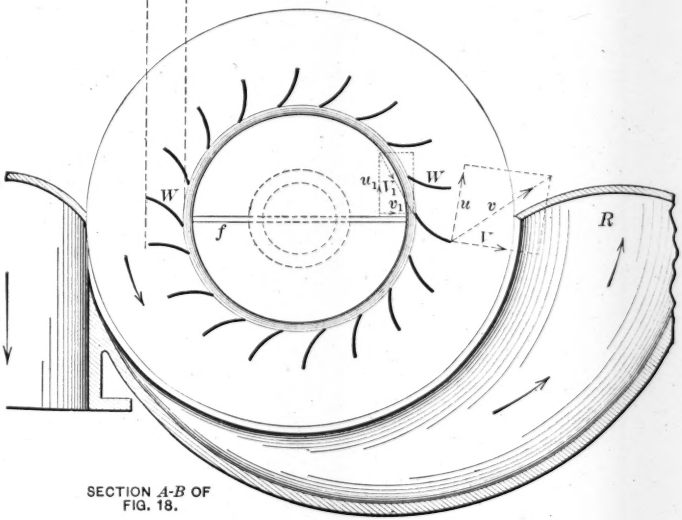
Notice that Equations XI and $XIII$ should not be applied except under the conditions specified in deriving Equation XI. These conditions, however, will be common ones in practice.

Suction Head in Air-Chamber.—On page 192 it is stated that the pressure should not change while the water is passing out between



OUTWARD-FLOW IMPULSE PUMP.

FIG. 18.



SECTION *A-B* OF FIG. 18.

FIG. 19.

the vanes. The pressure which actually exists is p; that fixed by the suction in the throat, m. (The remarks on page 185 apply in this case as well as in centrifugal pumps.) In this case the pressure, p, reaches the entrance to the vanes, and, by Equation *XII*, v_1 depends on p; k being assumed as fixed. Theoretically, $p = h - \frac{v^2}{2g}$ (neglecting friction and velocity in the discharge pipe).

It appears, then, that until we have some experimental data concerning p, the discharge of such pumps cannot be predicted accurately. Equations *VI*, *VII*, *VIII* and *IX* apply also to impulse pumps.

Areas of Passage Between Vanes.—Theoretically, we would get the same impulsive effect if the space between the vanes were only partly filled, as in Fig. 13 (*b*); but, to avoid all possibility of spraying, shocks, or sudden change of velocity, it is necessary that the water occupy the whole volume of the interior of the wheel, and issue therefrom in one unbroken sheet of uniform velocity around the circumference. In order that this condition be satisfied, and also that there be no change of pressure in the passage, it is necessary that the areas be proportioned carefully. Using the same symbols, and assuming that there are guide vanes at the entrance, in Fig. 14 (b), we have, since $v_1 \sin. \phi$ is the radial component of the velocity at the entrance,

$$v_1 \sin. \phi \times 2 \pi r_1 b_1 = Q \ldots\ldots\ldots\ldots\ldots\ldots (IX)$$

Whence we get b_1. Similarly, at the exit we have for the radial component: $V \sin. \beta$, or $v \sin. \theta$.

Hence,

$$V \sin. \beta \times 2 \pi r b = Q \ldots\ldots\ldots\ldots\ldots\ldots (VIII)$$

Whence we get b.

Evidently, in these, we assume that the vanes begin and end in a knife-edge. At any intermediate section of the passage the thickness of the vanes must be considered. The relative velocity at any intermediate section can be found by Equation *I*, *viz.*,

$$V^2_a = V^2_1 + u^2_a - u^2_1,$$

and the radial component is always

$$R_a = V_a \sin. \beta_a;$$

and

$$R_a (2 \pi r_a - n t) b_a = Q.$$

Whence b_a.

The subscript, a, indicates the intermediate section; n = number of vanes in the pump, and t = thickness of the vane at the point, a. This refinement need never be applied in practice.

There is no apparent mechanical principle, or practical difficulty, that would prevent the combination of centrifugal and impulsive effects. In that case the areas of the passageway need not be proportioned as carefully; but, on the other hand, since the pressures at the entrance and the discharge to the vanes would no longer necessarily be equal, the wheel, disk, or web, should be made solid, and air admitted to the opposite sides by separate tubes, as at t and t_1, in Fig. 16; otherwise the web of the wheel can be perforated as at g, in Fig. 18, and the pipe, t_1, omitted.

Proportioning.—For convenience, the necessary general formulas for designing impulse pumps are here reprinted:

$$V_1^2 = v_1^2 + u_1^2 + 2\ v_1\ u_1 \cos.\ \phi \quad \ldots\ldots\ldots\ldots \quad (X)$$

$$V^2 = V_1^2 + u^2 - u_1^2 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (I)$$

$$v^2 = V^2 + u^2 + 2\ u\ V \cos.\ \beta \quad \ldots\ldots\ldots\ldots \quad (II)$$

$$H = \frac{v^2 - v_1^2}{2\ g} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (XIII)$$

$$Q = 2\ \pi\ r_1\ b_1\ v_1 \sin.\ \phi \quad \ldots\ldots\ldots\ldots\ldots \quad (IX)$$

$$Q = 2\ \pi\ r\ b\ V \sin.\ \beta \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (VIII)$$

In parallel-flow pumps, r_1 must be taken as the radius to the center line of the inlet, and r that to the center line of the outlet to the vanes, and b_1 and b are the widths of the vanes, radially, at the inlet and outlet, respectively. See Fig. 16.

When ϕ and β are both 90 degrees:

$$v^2 = 2\ u^2 + v_1^2 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (XI)$$

$$u^2 = g\ H = \frac{v^2 - v_1^2}{2} \quad \ldots\ldots\ldots\ldots\ldots \quad (XIV)$$

$$Q = 2\ \pi\ r_1\ b_1\ v_1 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (IXa)$$

$$Q = 2\ \pi\ r\ b\ V \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (VIIIa)$$

The order of proceeding, to get the chief dimensions of an outward-flow impulse pump, would be about as follows: Q and H being given, k will be determined by considerations of convenience. The diameter of the entrance to the wheel will be fixed by the permissible velocity at the entrance, which may be 10 ft. or less; say v_z. Then $\pi\ r_z^2\ v_z = Q$, if the inlet is on one side only, or $2\ \pi\ r_z\ v_z = Q$, if on both sides. Thence, r_z. Then r_1 of the equations for the outward-flow pump should be a little greater than r_z, in order to provide a short curved approach to the vanes.

Next, we must decide on v_1. This depends on p, which is a matter of some uncertainty until some experimental data are collected. See

page 196. For the present discussion v_1 will be assumed as determined. Then b_1 is obtained from Equations *IX* or *IXa*.

The outer radius, r, may be fixed by considerations of convenience of construction, and whatever light experience throws on the subject. Undoubtedly, the vanes should be as short as possible, consistent with other necessary conditions. When a length of vane is thus fixed the next step should be to find a corresponding value of u that will give the necessary head. Unfortunately, the direct determination of u from Equations *X*, *I*, *II* and *XIII*, when H, v, β, and ϕ are given, is a difficult problem. For a single computation it would be easier to assume values of u and compute H, until, by trial, a sufficiently close value of u is found. If much work of this kind has to be done, tables should be prepared showing the relations between u and H for such values of β and ϕ as may be put in common use.

When u is determined, V can be obtained from Equations *X* and *I*, and then b can be found from Equation *VIII*.

Should it be determined to make the pump without guides, and to make $\beta = 90°$, then special Equations *XI*, *XIV*, *IXa* and *VIIIa* enable us to proceed directly.

The fixed vanes, e, are to stop the revolution of the water and direct it along the discharge pipe. Three such vanes should suffice.

The reaction, R, parallel to the axis, due to the discharge of the water from the wheel, will be, from the well-known laws of reaction:

$$\frac{WV}{g}$$

In the method by which Equation *I* is obtained for impulse pumps and motors, the analysis follows a single particle. This, of course, is not the actual condition, and the writer suspects that, in impulse pumps, and water-motors especially, the difference between the actual and assumed conditions leads to serious errors. To explain: Suppose the vane, $d\,e\,f$, Fig. 20, to move with the velocity, u; our analysis assumes only one thin lamina of particles following the surface of the vane, but, in practice, there would be a considerable depth normal to the vane, and each lamina is deflected by pressure (or reaction) from the next one beneath. Thus the lamina against the vane must be under greater

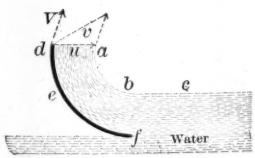


FIG. 20.

pressure than the outer one, and where the water reaches the discharge line, *d a*, that at *d* will escape with greater velocity than that at *a*.

It appears that the above circumstance will make our formulas for impulse pumps unreliable, and it must also reduce the efficiency seriously, for there must be considerable loss by friction when the laminæ of water move with high and low velocities side by side.

The best remedy the writer can suggest is to increase the number of vanes, to divide the water into thin laminæ, until the advantage thus gained is lost by increase of surface friction. Further discussion on this point will be found on page 204.

The foregoing discussion will not apply to centrifugal pumps, properly so called, for there the area of passage should be so large that, by thus reducing the velocity, all the liquid is brought to one condition as to pressure and velocity before escaping. See page 181.

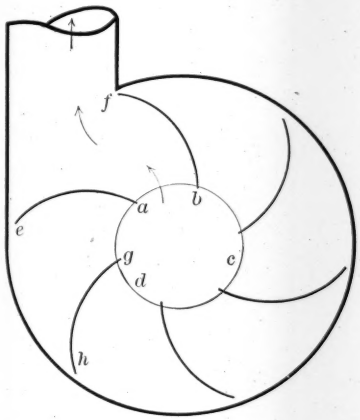


FIG. 21.

Fig. 21 shows diagrammatically what Mr. J. Richards calls an impulse pump. The writer is unable to see in it any advantage justifying its existence, but rather the contrary. One extraordinary condition in this design is that the flow between the propellers is not continuous. A glance at the figure will show that the discharge, during the instant in which any two propellers occupy the positions, *a e* and *b f*, is limited to the channel, *a b–e f*, and the velocity through *a b* must be high. The next instant, when *e* passes *f*, the outward motion of the water in *e a b f* must be stopped, and, in the same time, that in *h g a e* must be given the outward motion. It is, indeed, a thing of impulses. Surely such conditions are not favorable to smooth working and high efficiency.

Empirical Coefficients.—In all that has preceded, perfect efficiencies have been assumed, in order to present the underlying principle

clearly. Every reader knows that results computed by the various formulas cannot be realized in full. Experience alone can supply the coefficients necessary to make them sufficiently correct for practice. Until some experiments are available, the following may give some light on this matter:

After a wheel is built, as the lift depends only on the peripheral velocity, u, by computing u for a head greater than that specified, allowance can be made for imperfect efficiency. For instance, if the efficiency in lift is known to be 75%, and u is computed for $\frac{4}{3}$ H, we will get the head required.

In the matter of discharge, it is known that the coefficient of discharge from well-rounded nozzles is very nearly unity. Hence, it is probable that the equation for fixing the dimensions of the passage will give results which are nearly correct when the approaches are rounded properly.

Practical Limitations.—A little inspection of the equations, and the solution of a few numerical examples, will reveal the unfortunate fact that these pumps, either centrifugal or impulse, in which the pressure in the air-chamber is below that of the atmosphere, are not applicable when the quantity of water to be pumped is small, while the head is high; the reason being that the width of the peripheral discharge slot becomes impracticably small. Just where this limit will be drawn in practice the writer will not undertake to say. See page 202.

Example.—Proportion a pump to lift 20 cu. ft. per second through 10 ft.

Let the entering velocity to the wheel, $v_x = 15$ ft., and to the vanes, $v_1 = 10$ ft.; then the area of the circular entrance must be 1.33 sq. ft., or the diameter nearly 15½ ins.

Now, if we make $r_1 = 9$ ins., it will give 1¼ ins. for curving the approach to the vanes. Then, by Equation *IX*, b_1 is found to be nearly 5 ins. If ten vanes are put in they will be nearly 5½ ins. apart at the entrance. If they are made 4 ins. deep, radially, r becomes 13 ins. Then, by Equation *VIIa*, it is found that $u^2 = 320$, or u should be about 18 ft. per second, or about 2.6 revolutions per second. From Equation *VI* (assuming $k = p$), $V = 18$, nearly, and from Equation *VIIIa*, $b = 0.2$ ft., or nearly 2½ ins.

Evidently, then, such a pump would pass any solid in which the greatest dimension does not exceed 2½ ins. It may be worth while to

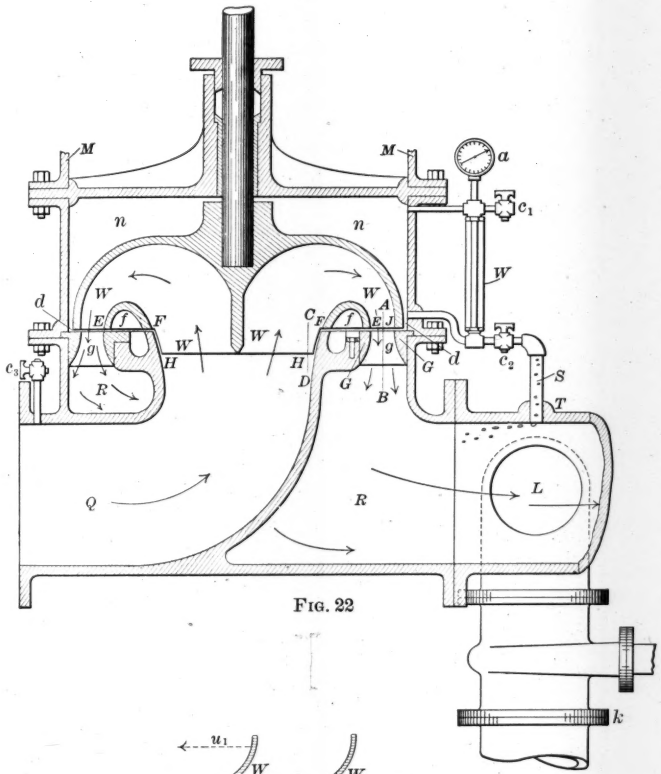
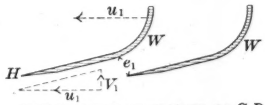
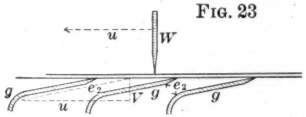


FIG. 22



DEVELOPMENT OF INTAKE VANES, ON *C-D*.

FIG. 23



DEVELOPMENT OF FIXED VANES, *g*, ON *A-B*.

FIG. 24

notice that, should any solid, too large to pass, get into the pump, it will cause no break nor even a grinding on any surface, but will merely lodge in the wheel and turn with it.

It is stated, on page 189, that in proportioning pumps to work as there assumed, the width, b, of the annular discharge slot would sometimes be found impracticably small; and, for that reason alone, the scheme could not be applied in cases where the quantity required is small and the lift great, as compared with conditions under which centrifugal pumps usually work. This difficulty can be overcome by devices now to be described.

The chief purpose now in view is to surround the propelling wheel with air at a pressure above that of the atmosphere, so that the necessary conditions,

$$\frac{v^2}{2g} + p = h,$$

may be fulfilled when $\frac{v^2}{2g}$ is less than h. Or, in other words, so that the velocity of discharge, v, can be reduced and the discharge area, $2\,\pi\,r\,b \sin.\,\beta$, be increased accordingly.

For accomplishing this purpose, Fig. 22 shows a scheme which has some additional points of advantage. In this, the air in the chamber, n, is not taken directly from the atmosphere, as in the previous propositions, Fig. 11. It is first taken into the intake or suction pipe, Q, through a cock c_3 (or, if the water is naturally gaseous, c_3 may be closed or omitted). The air then passes through the pump as entangled bubbles, and is drawn out of the discharge pipe wherever most convenient, and permitted to enter the chamber, n, through the pipe, S, the rate of passage being controlled by a cock, c_2.

That the above-described action can be obtained will be apparent, from the following:

The pressure of the air in the discharge pipe will be that due to the remaining lift, h, while that in the chamber, n, must be the same as the pressure in the water as it passes the open joint, d. Now, by well-known laws of hydraulics, the pressure at the joint, d, where the velocity is high ($v^2 = V^2 + u^2 + 2\,u\,V \cos.\,\beta$), cannot be as great as that in the discharge pipe where the velocity is low (the slight difference of elevation being neglected). Hence there will be a tendency for the fluid to flow through the pipe, S, and the cock, c_2, into n. The pipe, S, of course, should tap the discharge pipe at a point where the air tends

to collect. A very small stream of air entering c_3 will suffice, for all that is necessary is to replace the leakage of air through the stuffing-box. The circulation can be controlled by adjusting the cocks, c_3 and c_2. Any surplus of air will pass off through the discharge pipe. The water gauge, *W*, will show whether or not the chamber, *n*, is free of water. The pressure gauge, *a*, is not essential, but should always be attached, as it gives valuable information.

The advantage of this scheme, as announced at the outset, is that the width, *b*, or *E J* of Fig. 22, is not restricted; but, where high efficiency is the object, the fixed deflecting vanes, *g g* (Fig. 24), should be inserted to turn the water downward, thus stopping its revolution and checking its velocity accordingly, as soon as it leaves the propeller (more fully explained later). This will prevent what might be serious loss due to fluid friction.

In this design the propeller vanes, *W W*, extend to the center (though this is not essential). By this means, it is believed, the water will be taken up with less shock, and can be brought to a state of steady rotation before discharge, with more certainty and with fewer propelling vanes. With this intention, the designer should see that the vane everywhere parts the approaching water without conflict. To meet this condition the vanes should be vertical at the center and have a gradually increasing forward pitch at the lower edge as the distance from the center increases.

Notice that there is no tendency for the water to flow back through the joint, *E F*, for the water at *F* is already under centrifugal force, and hence there is as much tendency for the water to flow out through *F E* as to flow back.

The design, Fig. 22, is the writer's study, to combine, in the highest degree, compactness, simplicity of castings, and efficiency. Some other designs embodying the chief features are shown in Figs. 30 and 32.

When this style of pump is to be applied in dredging, the deflecting vanes, *g g*, should be omitted, the propelling vanes, *W*, made few and strong, and the receiving chamber, *R*, be made a "volute," that is, the area of *R* should increase progressively in the direction of revolution, and the water in it should move in the direction of revolution only. Thus modified, there is no apparent reason why it should not give excellent results as a dredge pump. Note that grit cannot get to the bearings, and that the area of discharge is unrestricted.

Concerning the pressure, p, in the chamber, n, the following may be interesting:

A necessary relation is $\frac{v^2}{2g} + p = h$.

Doubtless, such pumps will usually be made with the outer tips of the propelling vanes perpendicular to the direction of motion ($\beta = 90°$); then $v^2 = V^2 + u^2$. Substituting, we get

$$\frac{V^2}{2g} + \frac{u^2}{2g} + p = h.$$

Now, the object of this design is to make V small, and it should be less than 8 ft. On this assumption, $\frac{V^2}{2g}$ will be less than one. Hence, for the present purpose, it may be neglected. Remember, also, that $\frac{u^2}{g} = H$. (See pages 174 and 175. Substituting this, the relation becomes $p = h - \frac{H}{2}$, approximately.

Theoretically, the equations derived heretofore will apply to this design; but, practically, they are of little value (except the relation, $u^2 = g\,H$), for in this case a convenient value for V (4 to 8 ft. per second) can be assumed, and the parts proportioned consistently.

That the assumed V may be realized, the actual effective lift must be something less than the maximum theoretic lift, H, reduced by whatever friction losses occur beyond the open joint, d. This is a matter that cannot be formulated. Experience alone can give light upon it.

Relation of Vanes to Each Other.—In the preceding discussions it has been pointed out repeatedly that the edges of the vanes should be set so that the passing water will be parted without shock or "eddies," that is, they should be in the plane of relative motion. We have now to consider the best relation of one vane to the next.

It has also been pointed out that the common forms of centrifugal pumps lose, by "drag" at the tips of the propellers, most of the energy due to velocity ("kinetic" energy), and that this is just half of the energy in the water. It is therefore well to give some study to a means of better utilizing this kinetic energy.

The writer proposes that the vanes should be so related as to form flaring nozzles, of the best possible form consistent with other essential conditions of construction, and this should be done both in the starting or intake vanes and in the fixed deflecting vanes at the discharge.

In support of this opinion, attention may be called to the fact, well established by experimental hydraulics, that the efficiency of a properly proportioned discharge nozzle (water flowing toward the contraction) is about 98%, and, while recorded experiments on receiving nozzles (water flowing away from contraction) are not as numerous or varied, it is rational to presume that the action can be reversed, in correctly designed nozzles, without serious loss.*

Figs. 23 and 24 show vanes designed to act as receiving nozzles to go in the pump shown in Fig. 22. Note that the inclination of the vanes on the side from which the water approaches is such that the motion of the water is not affected until it enters the nozzle, *e*. For instance, in Fig. 23, which shows the conditions at the entrance, the water has only a low vertical velocity, which continues undisturbed as it rises under the helicoidal surface of the vane until it is struck and taken in by the orifice, *e*; then, being in the revolving nozzle, that is, between the vanes, it must take the revolving motion quickly. Here, the nozzle moves against nearly still water, while, at the discharge, Fig. 24, the water moves against fixed nozzles. The relative action is similar in the two cases.

In this design the discharge will vary with the velocity (rate of revolution); then, knowing the peripheral velocity necessary to give the lift, the inclination of the vanes must be computed accordingly.

Example.—Proportion a pump, of the type shown in Fig. 22, with 14-in. intake, to lift 8 cu. ft. of water per second against a head of 48 ft.

First, to find the peripheral velocity, u: If an efficiency of 75% be assumed, the pump must be designed for a speed which would give 64 ft. lift, theoretically. Then, by Equation *VII*, $H = \frac{u^2}{g}$, $u = 45$ ft.

* On this subject, the writer was somewhat discouraged and puzzled by that portion of the "Lowell Hydraulic Experiments" relating to discharge through a flaring ajutage (nozzle), in which it is pointed out that fifteen-sixteenths of the energy of discharging water was lost in the tube, but there is nothing irrational or discouraging about it when we are reminded that the absolute loss due to friction is independent of pressure. Hence, if there had been 100 ft. of head on the nozzle, instead of 1 or 2 ft., with the same velocity, the percentage of loss would have been small. To satisfy himself on this question, the writer prepared the apparatus shown in Fig. 25 and tested the efficiency of the combination under various conditions.

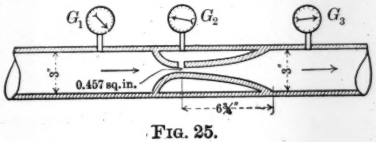


FIG. 25.

The records of two typical cases are as follows:

1.—Pressure head at $G_1 = 167$ ft.; at $G_2 = 0.0$; at $G_3 = 138$. Efficiency = 82.5%

2.— " " " $G_1 = 149$ " ; " $G_2 = -28$; " $G_3 = 117$. " = 78.5%

In the first the velocity at the throat was approximately 102, and in the second, 106 ft. per second. Doubtless, for less extreme cases, higher efficiencies can be obtained.

per second (nearly), and this may be taken as the velocity of the center line of the discharge zone. Assume the diameter of this center line to be 24 ins. Then the revolutions per second will be 7.16. The velocity of the outer limits of the intake vanes, W, at H (where the radius = 7 ins.), will be $\frac{7}{12}$ of 45 = 26 (nearly), and, the vertical velocity of approach being 8 ft., the inclination of the vane at this point must be 8 vertical to 26 horizontal (4 to 13). The inclination becomes more nearly vertical as the center is approached, being vertical at the center.

Assuming the width of the discharge zone to be 4 ins., then the area is 1 sq. ft. (nearly), and, therefore, the vertical component of the velocity of discharge (or the velocity, relative to the revolving wheel) will again be 8 ft. (nearly). Hence, the fixed vanes at discharge must be inclined 8 vertical to 45 horizontal (1 to $5\frac{1}{2}$).

Some convenient formulas for proportioning pumps of the class shown in Fig. 22 are as follows:

Let S = slope, or tangent, of the inclination of fixed deflecting vanes, g;

" d = radial width of same = width of discharge zone;

" r = radius of center of discharge zone;

" u = velocity of rotation at radius, r;

" V = velocity of discharge relative to the revolving wheel = vertical component of absolute velocity of discharge;

" a = area of discharge zone = $2\,\pi\,r\,d$.

Then $V = u\,S$, and $Q = a\,V = a\,u\,S$.

Similarly, let S_1, r_1, u_1 represent the slope, radius and rotative velocity, respectively, of the intake vanes at the outer limit; and let a_1 represent the area, and V_1 the velocity, in the intake.

Then $V_1 = u_1\,S_1$; $u = \frac{r_1}{r}\,u$; $a = \pi\,r^2_1$; $Q = a_1\,V_1 = a_1\,u_1\,S_1$.

Equating the two values of Q, we get

$$r\,a\,S = r_1\,a_1\,S_1.$$

Another equation easily proved is:

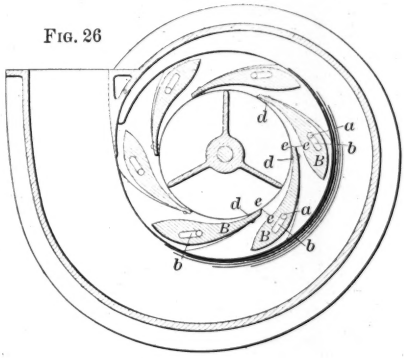
$$r^3_1\,S_1 = 2\,r^2\,d\,S.$$

In such a pump, run at varying speeds, the discharge will vary directly with the speed; the lift with the square, and the work with the cube, of the speed. Evidently, then, such a pump cannot be adapted to varying conditions. This has probably been the greatest handicap

to a more general use of centrifugal pumps; therefore, it is exceedingly desirable, in connection with other improvements, to make pumps of the best class adjustable.

Figs. 26, 27, 28 and 29 show the writer's design for an adjustable centrifugal pump. The discharge is dependent on the area of the orifices, *e e*, and this is controlled by swinging the vanes, *B*, about the

FIG. 26



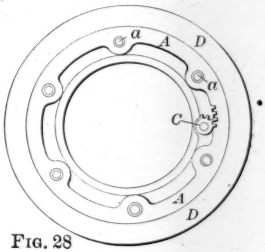
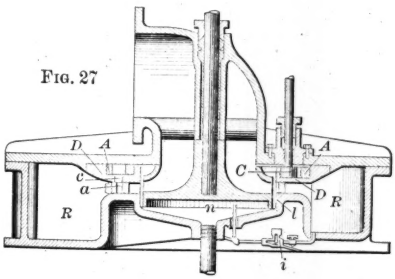


FIG. 28

FIG. 27



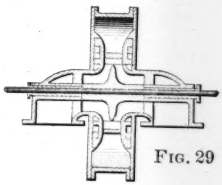


FIG. 29

hinges, *d*, thus affecting the opening, *e e*. From the adjusting ring, *A*, Figs. 27 and 28, the pins, *a*, *a*, etc., protrude through short slots, *c c* which are cut through the shell, *D*, on the circumference of a circle concentric with the wheel. These pins, *a*, engage the vanes, *B*, by entering the straight slots, *b*, cut in the vanes. When *A* is revolved, the pins, *a*, follow the slots, *c c*, and *B* must swing in or out accordingly. *A* is revolved through the necessary angle by the mechanism shown at *C*, Figs. 27 and 28, which may be duplicated on the opposite side. Fig.

26 shows the vanes, *B*, in place, with the upper plate removed. Fig. 28 shows the adjusting ring, *A*, lying in place. The inner curve of the vanes, *B*, is designed to maintain unchanged both the radial and revolving motion of the water; but, after it passes *e e*, into the flaring nozzle, the velocity is checked, and the pressure is increased accordingly.

The advantage of the down-and-out flow is in the opportunity it gives to support the weight of the wheel, shaft, etc., on high-pressure air from beneath, that is, in the chamber, *n*, In this case, where air occupies the space, *n*, below the wheel, the pressure is uniform, not being affected by the centrifugal force of the water revolving under the wheel, as assumed on page 184; hence, in this case, the uplift will be $\frac{62.5\ \pi\ r^2\ u^2}{2\ g} = 3\ r^2\ u^2$ (nearly), in pounds, when r is taken in feet. This is double the uplift obtained when water is revolving under the wheel—a point well worth notice in designing either pumps or water-wheels to operate on vertical shafts. Air can be conducted into this chamber by the means described on page 202, or by an inspirator, *i*, as shown in Fig. 27. The water for operating the inspirator can be drawn from any point in the discharge pipe. Note that the pressure in *n* is that at the open joint, *l*, at the tips of the vanes, where the velocity is highest, and, therefore, the pressure in *n* will be less than in *R* (one-half, theoretically).

Fig. 29 shows the outlines of an adjustable, double-intake pump on a horizontal shaft; the adjustment to be made substantially as in the preceding. Such a design has very manifest advantages for a permanent plant to handle large quantities of water from a nearly permanent and easily accessible level.

It is noteworthy that the designs discussed on pages 206 to 208 have every element of a first-class "reaction" water-wheel (turbine), and, operated as such, will probably give good results.

Compounding.

The principle involved in compounding centrifugal pumps is simple enough. It consists in taking the water as delivered from one pump and passing it through another, thus getting the effect of each pump independently. But, when the designer takes up the problem, he finds an infinite variety of possible combinations, from which he must select

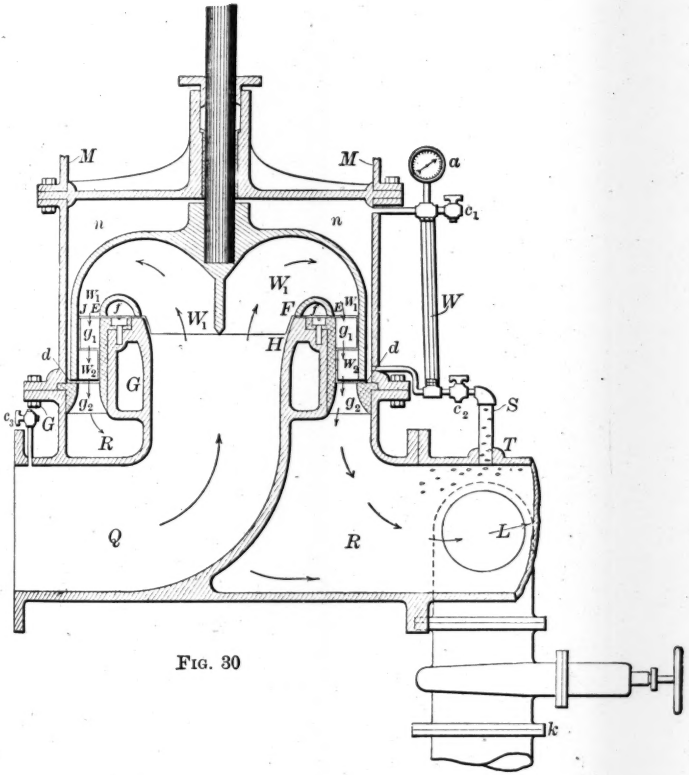

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FIG. 30

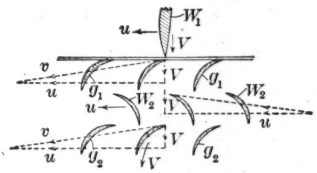
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FIG. 31

that one which is most advantageous, everything considered, and he must consider, chiefly, compactness, simplicity of castings and efficiency of operation.

After studying the subject in the light of all that precedes in this paper, the writer offers the design for a compounding pump shown in Figs. 30 and 32.

Having explained all other new features in this design we have now only to explain the features involved in compounding.

The construction and operation, out to the lower edge of the fixed deflecting vanes, g_1 g_1, is identical with that in the single pump, Fig. 22. At this point the water is flowing vertically and has a low velocity (all the motion of revolution having been checked by the vanes, g), and its pressure is that due to whatever head the single pump could produce. The water now comes in the path of a second set of propelling vanes, W_2 W_2, which are attached to the same revolving piece that carried the first set, W_1 W_1, as is shown plainly in Fig. 30. This second set of vanes must be set so that the upper edge will part the water without shock—which is a matter of simple geometry. The water, in gliding between the vanes, W_2, partakes of their velocity, and, hence, at the line of escape, or lower edge of W_2, the water again has a high velocity, as shown by the dotted lines in Fig. 31, but, immediately after its escape from W_2, the water comes against a second set of fixed deflecting vanes, g_2, by which the component, u, of its velocity is again checked and the pressure increased correspondingly, according to the well-known law of hydraulics.

Thus, the pressure can be increased indefinitely by a succession of moving and fixed vanes.

It can be shown (see page 194) that the effect of the vanes, W_2, will be exactly equal to that of the first set of vanes, W_1, which act more nearly under the laws of purely centrifugal force.

The details of construction can be varied indefinitely, but the simplicity of that shown in Fig. 30 is not likely to be surpassed unless it be by that shown in Fig. 32.

Note that in Fig. 30 the area of the horizontal section of the water channel is constant between the first and last lines of discharge. Hence the vertical component, V, of the velocity must be constant. This simplifies very much the construction and also the problems in proportioning. The flow, of course, could be made radial and yet

retain the novel feature here described, but the space occupied by the pump would be increased very much and the construction complicated.

Fig. 32 shows a compound turbine* pump, which will now be readily understood. The water enters from above, through Q. The space, n_2, will be occupied by air below atmospheric pressure (if Q is under suction), the air being admitted through a cock on the pipe, t_1; the air thus admitted doing, also, the duty of that admitted through c_1 of Fig. 30. The space, n_1, will be occupied by air at high pressure admitted through the pipe, S, by the process described in connection with Fig. 22.

This design has one additional and noteworthy advantage, in that the high pressure is under the wheel, and all the reaction of the flowing water is upward. Thus the weight of the wheel and a shaft of length equal to the lift can usually be fully supported, and often more than supported.

As an example of the theoretic proportions of pumps of the style shown in Figs. 22, 30 and 32, the following is offered:

Assume an intake pipe, 8 ins. in diameter, and an intake velocity of 8 ft. per second. This gives 2.8 cu. ft. per second. Assume the outer diameter of the discharge (to J) = 13 ins., and the inner diameter (to E) = 10 ins., or $b = 1\frac{1}{2}$ ins. Then the discharge area = 0.376 sq. ft., and the vertical component of the velocity there = 7.4 ft. per second, and this does not change until the horizontal area changes.

Assume 800 revolutions per minute. Then the velocity, u, of the outer tips of the vanes will be nearly 42 ft. per second, and the theoretic lift, $H = \frac{u^2}{g}$, will be nearly 50 ft. when the outer tips of the propelling vanes are perpendicular to their motion, that is $\beta = 90$ degrees.

Under the above conditions, the inner tips of the vanes, where they take up the water at H, should slope $4\frac{1}{2}$ horizontal to 1 vertical, as in Fig. 23, and the top edge of the fixed deflectors, g, should slope in the opposite direction about 6 to 1, as in Fig. 24.

The extreme outside diameter (over the flange) of such a pump would be about 21 ins. The figures are drawn to scale and show the proportionate heights.

* This non-committal name is used because the action is neither purely centrifugal nor impulsive.

The lift found for a single-acting pump can be doubled (theoretically) by compounding, as in Figs. 30 and 32, which are drawn to scale and show how little additional weight or work would be necessary to accomplish that result.

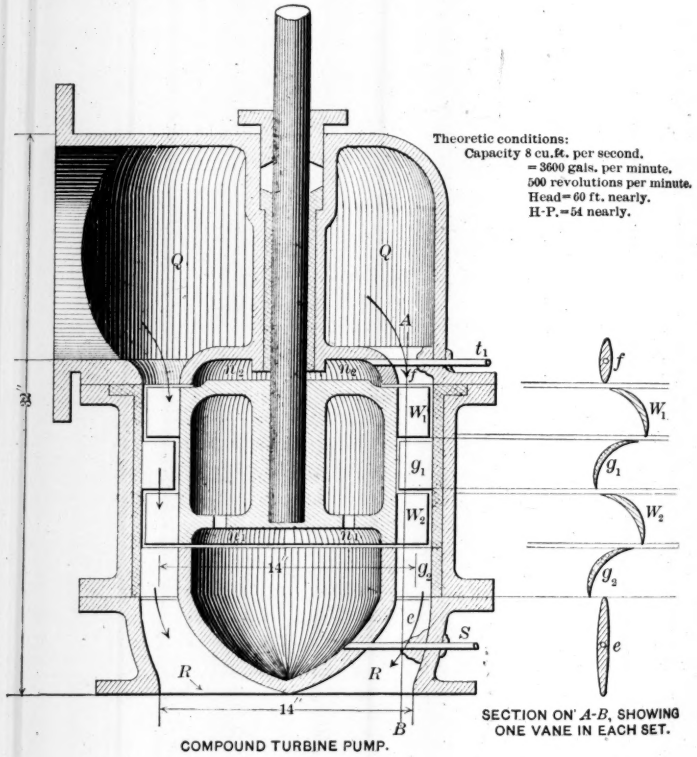


FIG. 32.

FANS.

The demonstrations and formulas heretofore given, relating to both centrifugal and impulse pumps, apply to any fluid of constant density. Notice that weight, or density, appears in none of the formulas for dimensions, velocities or heads. If, then, we enter the study of fans or blowers, assuming that the change of density of the air (or any gas) while passing through the machine is so slight that it may be

neglected, we can accept without question any of the formulas offered for either centrifugal or impulse pumps. Indeed, it has already been stated that in impulse pumps there should be no change of pressure in the fluid as it passes through the vanes. Then the formulas for impulse pumps apply to fans, without any qualifications.

It will be shown presently that the simplest, cheapest and probably the most efficient fan will discharge under atmospheric pressure. It will now be shown that in that case, also, the equations will stand, although centrifugal force may cause a compression within the wheel.

In Fig. 1 assume the cylinder, $C\,B$, filled with a compressible fluid, as air, then the weight of a unit volume will not be constant, but will depend on the distance, x, from the center.

Let W' represent this weight at the distance, x, from the center. Then the centrifugal force, due to a disk of unit area and infinitesimal thickness, $d\,x$, will be

$$d\,f = \frac{W'\,u_x^2}{g\,x}\,d\,x = \frac{W'\,u^2}{g\,r^2}\,x\,d\,x, \text{ since } u_x = \frac{x}{r}\,u.$$

Also,

$$\frac{W'}{W} = \frac{f}{p_a}, \text{ where } f = \text{the absolute pressure in the fluid at the distance, } x, \text{ from the center, and } W \text{ and } p_a \text{ are the weight and pressure, respectively, of free air.}$$

Substituting and dividing by f we get

$$\frac{d\,f}{f} = \frac{W\,u^2}{g\,r^2\,p_a}\,x\,d\,x.$$

Then

$$\int_{p_a}^{p_1} \frac{d\,f}{f} = \frac{W\,u^2}{g\,r^2\,p_a} \int x\,d\,x.$$

Whence,

$$\log.\ \frac{p_1}{p_a} = \frac{W\,u^2}{2\,g\,p_a\,r^2}\,(r^2 - r_1^2)$$

Whence p_1 is the pressure at the outer limit, B.

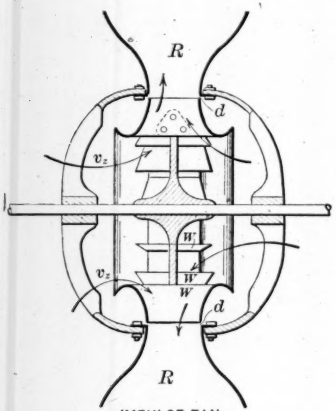
Now, assuming no change of temperature, it is known that whatever energy is pent up in the compressed gas within the wheel must be found as kinetic energy (energy of motion), when the gas escapes under atmospheric pressure. The energy in the gas within the wheel

is partly in the pressure and partly in the velocity of revolution. Hence we have the equation

$$p_a\ Q_a \log_e \frac{p_1}{p_a} + \frac{W\ Q_a\ u^2}{2\ g} = \frac{W\ Q_a\ v^2}{2\ g};$$

where v is the absolute velocity immediately after escaping. Substituting the value of $\log. \frac{p_1}{p_a}$, found above, the equation reduces to $v^2 = 2\ u^2 - u^2_1$, which expression will be obtained by the analysis on page 170 when the velocity of approach is omitted.

In the writer's opinion, fans should be designed and proportioned in accordance with the principle of impulse pumps, except that the



IMPULSE FAN.
FIG. 33.

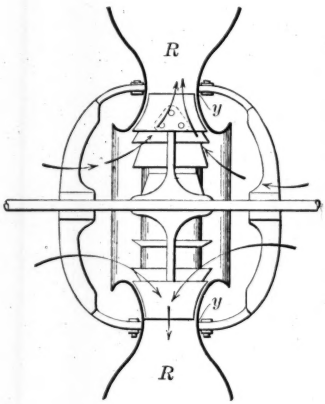


FIG. 34.

enclosing casing can be omitted. See Fig. 33. Such constructions have the following advantages:

(*a*)—They are composed of few and simple parts.

(*b*)—The vanes being short, radially, great inlet area can be provided in a fan of comparatively small diameter, and the discharge increased correspondingly.

(*c*)—The condition that the discharge is at atmospheric pressure renders the mathematical analysis of the action simple, and satisfactory formulas for proportioning and speeding a fan can be obtained.

(*d*)—The condition as to pressure (or suction) at the joint, *d*, reveals whether or not the wheel is speeded and proportioned properly.

Furthermore, these impulse fans (if that name be accepted) will, for reasons of simplicity and economy, probably be without guide vanes, and, for similar reasons, the moving vanes may be assumed to be radial at the outer end. It is apparent, further, that since the air enters and leaves at atmospheric pressure, the two factors, k and p, in the equations, disappear or balance each other. Thus simplified, the principle formulas become:

$$v^2 = V^2 + u^2 = 2\,u^2 + v_1^2 \ldots\ldots\ldots\ldots \quad (XI)$$

$$Q = 2\,\pi\,r_1\,b_1\,v_1 \ldots\ldots\ldots\ldots\ldots\ldots \quad (IXa)$$

$$Q = 2\,\pi\,r\,b\,V \ldots\ldots\ldots\ldots\ldots\ldots \quad (VIIIa)$$

As the air passes by the joint, d, its energy is all kinetic, or $E = \dfrac{W\,Q\,v^2}{2\,g}$, but after it gets into the discharge conduit its energy will usually be partly due to pressure and partly due to velocity. Thus, suppose there is required an absolute pressure, p_2, and a velocity, v_2, in the conduit; then, by the law of conservation of energy,

$$E = \frac{W_a\,Q_a\,v^2}{2\,g} = p_a\,Q_a\,\log_e.\,\frac{p_2}{p_a} + \frac{W_a\,Q_a\,v_2^2}{2\,g}.$$

Whence,

$$v^2 = \frac{2\,g}{W}\,p_a \log._e R + v_2^2 \ldots\ldots\ldots\ldots\ldots\ldots \quad (XV)$$

Where log. R = hyperbolic log. of $\dfrac{p^2}{p_a}$; W = weight of air = 0.076 lb. when the temperature is 60° Fahr., and $p_a = 2\,116$ lbs. per square foot. Inserting the numerical values, we get

$$v^2 = 1\,782\,000 \log.\,R + v_2^2 \ldots\ldots\ldots\ldots\ldots\ldots \quad (XVI)$$

While Equation *XVI* is theoretically correct, it is the common practice to neglect the effects of compression, and work by equations identical in form with those used in hydraulics, differing only in the weight of the fluid and in the coefficient of friction. This practice is sufficiently accurate for practical purposes, within the ordinary limits of compression produced by fans. Thus, instead of the equation above for E, the equation for energy would be written

$$E = P\,Q_1 + \frac{W\,Q_a\,v_2^2}{2\,g} = \frac{W\,Q_a\,v^2}{2\,g};$$

where P is the pressure above the atmosphere, and Q_1 is the volume at the pressure, P; but, if compression is neglected, Q_1 may be taken $= Q_a$; then Equation *XVI* reduces to

$$v^2 = \frac{2\,g\,P}{W} + v_2^2 \ldots\ldots\ldots\ldots\ldots\ldots \quad (XVII)$$

or, inserting numerical values, this becomes

$$v^2 = 842\ P + v_2^2 \ldots\ldots\ldots\ldots\ldots\ldots (XVIIa)$$

Results computed from Equations *XVI* and *XVIIa* will not differ enough to affect the dimensions in designing. Hence, the latter, being much simpler, is preferable for that purpose; but, in computing the efficiency of a fan, Equation *XV* should be applied, and a value of W inserted, according to the temperature of the passing air.

Example.—Proportion a fan to deliver 400 cu. ft. per second of free air (at a temperature of 60° Fahr.) into a passage where the pressure is 2 ins. of water (10.4 lbs. per square foot), the velocity to be 16 ft. per second ($= v_2$). See Fig. 33.

$$R = \frac{2\,116.8 + 10.4}{2\,116.8} = 1.005;$$

$$\text{Log.}_e\ R = 0.005;$$

and

$$v_2^2 = 256.$$

Then, by Equation *XVI*,

$v^2 = 1\,782\,000 \times 0.005 + 256 = 9\,166$, or $v = 96$, nearly.

By Equation *XVIIa*,

$v^2 = 842 \times 10.4 + 256 = 9\,012$, or $v = 95$, nearly.

Evidently, either result will be sufficiently accurate for designing.

Now, if an entrance velocity, z, of about 60 ft. per second is allowed, the area of the entrance $= \frac{400}{60} = 6.66$ sq. ft., or 3.33 sq. ft. on each side. The area of a circle of 1 ft. radius is 3.14 sq. ft. Hence we may take $r_1 = 1$ ft., making $v_2 = 63$.

Assuming the velocity at the entrance to the vanes, v_1, to be the same as z, we have,

$$Q = 400 = 2\,\pi\,r_1\,b_1\,v_1,$$

whence

$$b_1 = 1 \text{ ft., very nearly.}$$

Now, if there are twelve vanes, they will be about 6 ins. apart at the inner tip. Then a radial length of 6 ins. for the vane seems to be sufficient. This will make the fan wheel 3 ft. in diameter.

By Equation *XI*,

$$u^2 = \frac{v^2 - v_1^2}{2} = \frac{1}{2}(9\,012 - 3\,960) = 2\,512, \text{ and } u = 50, \text{ nearly.}$$

Also, by Equation *III*,

$$V^2 = v^2 - u^2 = 9\,012 - 5\,043 = 3\,969, \text{ and } V = 63.$$

Then, by Equation *VIIIa*,

$$400 = 6.28 \times 1.5 \times 63 \times b,$$

Whence

$$b = 0.66 \text{ ft.} = 8 \text{ ins., nearly.}$$

The vanes should curve forward at the inner end to make an angle with the tangent, of which the tangent, $\frac{v_1}{u_1} = 1.31$, or the angle $= 52° 45'$.

The theoretic horse-power of such a fan would be

$$\frac{W\,Q,\,v^2}{2\,g \times 550} = 8, \text{ nearly.}$$

Fig. 33 is drawn to scale showing the dimensions as just computed. The vanes and the hollow wheel in which they are inserted can best be made of sheet metal polished inside and outside.

The curved approach to the inlet should not be omitted. Judging by similar conditions in hydraulics, the discharge will be about 50% greater with a curved inlet than without. Hence, evidently, this is one of the most important details.

Adjustment.—The joint, *d*, is open to the atmosphere. When the wheel is designed and speeded according to principles already discussed, the energy passing the joint, *d*, is all kinetic, so that there is no tendency to leakage inward or outward. However, in most cases in practice, the resistance to be overcome in forcing air through the passage (pipe, mine, tunnel, building, furnace, or the like) cannot be computed accurately, and may vary from time to time. Hence, in order to meet these conditions satisfactorily, it is desirable that the fan should be adjustable. In order to understand this phase of the subject, the following discussion is necessary:

If all effects of compression are neglected, which may be done under the ordinary conditions of practice, the resistance to be overcome when forcing air through a passage, in which the conditions are constant, will vary with the square of the velocity (proved by experiment), and, of course, the velocity varies directly with the quantity of air passed. Hence the resistance varies with the square of the quantity. Also, from Equation *VIII*, it is seen that Q^2 varies with V^2, all other factors being constant; and, by Equation *VI*, V^2 varies with u^2. Hence, from the foregoing considerations, it may be said that the resistance varies with u^2; and, from Equation *XI*, the pressure head produced by the wheel,

$$H = \frac{v^2}{2\,g} = \frac{2\,u^2 + v_1^2}{2\,g},$$

showing that it is sufficiently accurate for the present purpose to say that *H*, or the propelling force, varies also with u^2, v_1 being comparatively small.

Thus it is found that a fan cannot be adjusted to a variable resistance by varying the speed only. The folly of attempting to do so will be still more apparent when it is considered that the necessary power to run the fan varies with the cube of the speed, if it has a free discharge.

The problem of adjustment, then, is reduced to this: Either change the resistance by changing the quantity of air delivered without changing the speed, or change the speed (and thereby the propelling force) without changing the quantity; or, possibly, both may be changed at once. In any case, it would be necessary to make the area of the air passage adjustable. Theoretically, the adjustment ought to be made in the area of the discharge passage from the wheel; that is, b, in the equations, should be adjustable; but, practically, it is very doubtful if the advantage would outweigh the disadvantage of mechanical complications necessary to provide for such adjustment. The discharge could be adjusted as in the adjustable pump. See page 207.

The area of the inlet could easily be throttled to any extent, and some provision to do so may be found advantageous, though, theoretically, it is not the proper place to apply the adjustment.

Attention should be called to the fact that the conditions at the joint, d, will indicate to the attendant whether the speed and volume of discharge are properly related to the resistance, that is, if air is escaping at d, the resistance is too great, or the wheel is discharging more air than it can force through the passages; while, if air is being drawn in at d, the resistance is less than the wheel could overcome, or the wheel could send more air through the passages.

The writer suggests that, until experience throws light on this part of the study, the area of the discharge slot be made a little less than computed. Then, by speeding up, the required quantity can be obtained, and there will be some excess of propelling head. Without this suggestion it is probable that the designer would suppose that in order to be on the safe side both area and speed should have some excess.

Fig. 34 represents a fan in which the vanes are open on the side, and turn within a fixed casing. Otherwise, the proportions should be worked out as for Fig. 33. This construction is not as simple or as economical as that shown in Fig 33, though it represents present practice more nearly. Nor would it indicate whether the proportions and

speed were properly adapted to the resistance. However, an orifice through the casing, as at y, on the line of least width, would give the needed indications, provided the proportions are in accord with the method described on pages 215 to 217. The idea is apt to prevail that by closing up the sides of the fan, as in Fig. 34, the air is kept in the passage and forced to move on into the conduit; but this would be an error, for if the propelling head, $\frac{v^2}{2g}$, and the possible volume are not properly related to the resistance, any excess of volume will not enter the fan. Attention to this fact will explain the paradox that, within assignable limits, the more open the passage in a mine the more power is required to run the fans. Close the passage, and no air passes. Then, though the fan continues to run, it discharges no air, and the power required is only enough to overcome the friction within the fan. This is one extreme. The other is, when there is no resistance outside the fan.

Testing Centrifugal Pumps.

Unfortunately, the published data taken in testing centrifugal pumps are incomplete in every case that the writer has examined. The only object in making the tests seems to have been to see if the pumps met the conditions of the contract. That, of course, is the most important matter, as far as a single pump is concerned, but what are most needed are comparative results as affected by variations in proportions, angles, speed and head. It is only from such data that we can interpret results intelligently and detect the combinations that give the best results.

The data which should be recorded in a test are:

First.—All dimensions and details of the water passages, including:

(*a*)—Suction pipe; length, diameter, valves, etc.;

(*b*)—Curves of entrance to the wheel, and details of the joint between the wheel and the casing when the runners are inclosed;

(*c*)—Angles (α and β) of the intake and discharge ends, respectively, of the propellers, with a tangent to the circle in which they move;

(*d*)—Details of the open joint between the runner and the casing at the discharge, when inclosed runners are used;

(*e*)—Details of the curve, if any, through which the water passes from the tips of the propellers into the "volute";

(*f*)—Dimensions of the "volute";

(*g*)—Conditions of the various surfaces as to polish.

All the above can be shown by two sections; one through the center in the plane of revolution, the other through the center at right angles to the plane of revolution. Dimensions should be shown in figures.

Second.—The pressures at three points, *a*, *b* and *c*, should be recorded, as follows:

(*a*)—In the suction, as near as possible to the wheel, but on a straight pipe or channel;

(*b*)—At the outer tip of the propellers, by tapping through the casing at right angles to the plane of revolution;

(*c*)—In the first straight section of the discharge pipe;

(*d*)—Relative elevations of the gauges at *a*, *b* and *c*.

Care must be taken to place the gauges where centrifugal force will not affect the results. When the gauges are set thus it will not be necessary to take the elevations of the water surface in the sump and discharge.

Third.—(*a*)—Speed, or revolutions per minute;

(*b*)—Water delivered;

(*c*)—Power consumed when discharging;

(*d*)—Power consumed with pump full of water, but without discharge;

(*e*)—Power consumed when pump is running empty.

Fourth.—The following points should not be overlooked in preparing for and in conducting a test:

(*a*)—The pump cannot start a discharge under a head that it can maintain after starting. Hence, a valve discharging under low head should be provided.

(*b*)—It will seldom be found practicable to deliver the water to a stand-pipe with the desired head, especially when it is desired to vary the head, as will usually be the case. Then the only recourse is to throttle the discharge beyond the pressure gauge in the discharge pipe. In doing this, remember that with the discharge cut off entirely the maximum pressure head will be $\frac{u^2}{2g}$, including suction, and, as the valve is gradually opened the pressure head will rise, theoretically, to a maximum of $\frac{u^2 + u\,V \cos.\,\beta}{g}$, including suction head, after which, if the valve be opened further, the head will become less. Thus

a skilful manipulation of the throttle valve while watching the pressure gauge should enable the operator to find readily the maximum lift for any speed. Theoretically, the condition that gives the greatest lift should also give the greatest efficiency. Practically, the greatest efficiency will probably be found when the valve is opened a little wider than when giving the greatest lift.

(*c*)—The total lift to be credited to the pump is the head shown on the suction gauge plus that on the discharge gauge plus their difference of elevation. The elevation of the gauges should be assumed at the surface of the water standing in the small pipe leading to the gauge, and some provision to determine this should be made.

APPENDIX.

While pursuing these studies, the following scheme for a hydraulic air-compressor occurred to the writer. It is a natural result of the ideas presented on pages 181 to 184.

Let *W*, Fig. 35, represent a centrifugal pump working in a closed circuit, *P Q R*, as shown; then, evidently, neglecting slight differences of elevation, the pressure in *Q* is equal to that in *R* (see also Figs. 11 and 18). How, then, can we account for the work done by the pump?

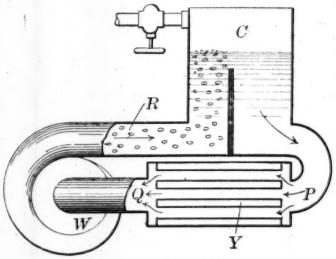


FIG. 35.

The answer depends on circumstances. For our present purpose we will assume an air space, *C*, somewhere in the circuit, and that free air has access to the joint, *d*, through the spaces, *n*, Figs. 11 and 18. Under these circumstances, the rush of water through the flaring nozzle, *m*, into a relatively unresisting field (since the pressure in *R* equals that in *Q*) will cause a suction at the joint, *d*, and, assuming proper provisions for the admission of air, the entering air will be compressed to the pressure in *R* as soon as the velocity is checked.

The air thus compressed can be separated in a chamber, *C*, and thence conducted away. Theoretically (as the word is usually applied), we should find the energy in the compressed air equal to the work done by the pump, there being no other channel through which the energy can disappear except by friction in its various forms.

The water would absorb the heat of compression, and therefore isothermal conditions would be very nearly realized, but the heat thus absorbed would have to be taken out by exterior cooling, as suggested at *Y*, Fig. 35.

Another attractive feature in the scheme is that, theoretically, with nearly constant motive power and speed, the degree of compression can be varied indefinitely and the quantity of air entering will be self-regulating.

An easy computation will show that a compressor of great power and volume can be embodied in a small machine.

Disregarding cost, if mercury be used instead of water, the calculations show remarkable results.

If the air-chamber be abolished, or filled with water, and water, instead of air, be drawn in at *d*, we have a hydraulic jack of unlimited power.

The practical difficulties in carrying out such a scheme will probably be serious, and possibly insurmountable. To test it sufficiently to say whether or not it can be made a success would require extensive and expensive experiments. The main questions are: Can enough air be taken into the current to absorb the energy; and what is the best mechanical means for providing for its admission?

DISCUSSION.

Mr. Venable. WILLIAM MAYO VENABLE, Assoc. M. Am. Soc. C. E. (by letter).—This paper is very interesting, as it points out some facts not generally understood; but the writer cannot agree with the theory advanced, which is based partially upon an erroneous assumption, and neglects the effect of the vanes acting in any direction other than that of rotation. The conclusion concerning the possible improvement of centrifugal pumps by adopting a type of pump with a nozzle, or contraction of the opening, at the tips of the blades, producing a high radial velocity of flow there, is entirely erroneous.

Apply the author's Equation *I* to the particular case used as an illustration on page 182, as follows:

$$V^2 - V^2_1 = u^2 - u^2_1.$$
$$6.8^2 - 8.6^2 = 57^2 - u^2_1;$$

whence u_1 must be greater than u, which is mechanically impossible.

The paper makes

$$V^2 = V^2_1 + u^2 - u^2_1,$$

or V is the velocity produced by a pressure head of

$$\frac{V^2_1 + u^2 - u^2_1}{2g} = \frac{v^2_1 + u^2}{2g}$$

discharging water through an orifice against no external pressure.

In order that the author's formulas may hold, the first condition must be that there shall be no pressure head whatever at the orifice at the tips of the blades. But any pump that discharges water against a head must produce a head equal to the head worked against and also a head sufficient to give the water the necessary velocity, v_1, to enter at the intake; that is, a velocity necessary to pass the required quantity through the smallest section of the suction passage, assuming that from there on the pump has no losses.

But the velocity, V, reduced to zero by enlarging the discharge passages, would only create head worked against, if the pressure at the tips of the vanes is zero; and this would leave no source of head to produce the flow at the intake, v_1. V, therefore, can never be as great as the formula requires.

Consequently, the formula,

$$v^2 = V^2 + u^2 + 2\,u\,V \cos.\,\beta,$$

cannot apply, because V is not a real velocity, but a hypothetical velocity, which is in reality represented by a less velocity, and an actual pressure head.

A proper understanding of the relation between velocities and pressures in a pump must be based on first principles.

"Centrifugal pumps," "screw pumps," "turbine pumps," and all other kinds of pumps in which the flow of water is unidirectional

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and uniform; that is, all pumps not provided with reciprocating parts depend upon increasing the velocity of the water by means of moving vanes or orifices and retarding that velocity by passing the water through a channel of enlarging section. Both the increase and the diminution of velocity are accompanied by the production of useful head. Water motors of the corresponding types only reverse the process, and develop mechanical motion.

If v is the velocity of motion of water past an orifice, normal to its opening, and if the pressure within is such that water does not enter, h is the head at the mouth of the orifice, and

$$v^2 = 2\,g\,h.$$

But, if h does not balance the tendency of the water to enter the orifice, and water flows in with a velocity, v_1, losing a velocity, v_2,

$$v = v_1 + v_2,$$

the velocity head remaining in the water is

$$h_1 = \frac{v^2_1}{2\,g},$$

and the pressure head at the orifice is

$$h_2 = \frac{v^2 - v^2_1}{2\,g} = \frac{v^2_2 + 2\,v_1\,v_2}{2\,g}.$$

Therefore, if h_2 and v are known, v_1 can be found, for

$$v^2_1 = v^2 - 2\,g\,h_2 = 2\,g\,(h - h_2).$$

If Q is the quantity entering the orifice per second, and a is the area of the orifice,

$$Q = a\,v_1 = a\,\sqrt{v^2 - 2\,g\,h_2} = a\,\sqrt{2\,g\,(h - h_2)}.$$

If the pump consists of this simple orifice without other devices affecting the flow or pressure (a purely ideal condition, mechanically impossible), the work lost in making water flow through the orifice is proportional to $Q\,v^2_1$, and the useful work is proportional to $Q\,(v^2 - v^2_1)$. The efficiency, therefore, is $\frac{v^2 - v^2_1}{v^2} = \frac{h_2}{h}$, and the loss ratio is $\frac{h_1}{h}$.

These formulas hold good, not only for a simple orifice, but for any device in the form of a fixed passageway where the water changes its velocity from v to v_1 without friction or internal currents; for instance, it applies to a passage which has a smaller section where the water enters than where it emerges, and to the openings between blades on ordinary centrifugal pumps.

Besides the loss due to h_1 there are also friction losses. These may be divided into three classes:

1.—Friction of water on the surface of the pump parts;

2.—Friction of water on water, due to ordinary changes in velocities which cannot be avoided;

3.—Friction of water on water caused by abrupt changes in the direction of flow, so that the water is disturbed unnecessarily, owing to imperfection in design.

Mr. Venable. Necessarily, the process of taking water at an orifice, if the water be assumed as stationary and the orifice moving, must be accompanied by a counter process, that of discharging the water through another orifice, and reducing its velocity again to rest. This implies two sets of orifices or passages, one set moving and one set fixed. In a pump, the motion of the water, relative to the passage in which it is flowing, should be a continuous process of retardation. Its actual velocity undergoes, first, a process of acceleration, and, second, a process of retardation. In a motor, the motion of the water, relative to the passage in which it is flowing, is a continuous process of acceleration. Its actual velocity undergoes changes similar to those in a pump, but with velocities reversed in direction.

An impervious surface moving in a liquid, neglecting friction, can produce a velocity only normal to its face. It must produce a velocity equal to the component of its velocity normal to its face, as otherwise the liquid would pass through the surface. The actual velocity of the liquid must be the result of the velocity of the motion of the surface, normal to itself, and of some velocity, parallel to the surface, not affected by its motion.

If the surface of the plate is normal to the direction of its motion, it produces velocity only in the direction of its actual motion.

A series of such surfaces arranged radially around an axis may be called a simple centrifugal pump.

Let r = radius at any point on the surfaces;

ω = angular velocity, in revolutions per second;

v = actual velocity of the surface at any point;

$v = 2 \pi r \omega$.

The centrifugal force is the result of restraining the velocities, v, into circular paths. If the water is at rest between r_1 and the axis of rotation, the centrifugal force due to rotation between r_1 and r_2 produces a head,

$$h = \frac{v^2_{r_2} - v^2_{r_1}}{2g}.$$

If there is no flow, the water will be set in motion from the axis out, and the head becomes

$$h = \frac{v^2_{r_2}}{2g},$$

which is the same as the $\frac{u^2}{2g}$ derived by Mr. Harris.

This head is the head produced by retarding the velocity, v_{r_2}, to 0.

The head, $\frac{v^2_{r_2} - v^2_{r_1}}{2g}$, is the head produced by retarding the water from v_{r_2} to 0, less the head required to give it the velocity, v_{r_1}, necessary to enter between the plates.

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Call the radial velocity through the pump w. The motion of rotation cannot affect this velocity, which is parallel to the blades. Therefore, w must be produced by the head, $\frac{v^2_{r_2} - v^2_{r_1}}{2g}$; leaving a head, $\frac{v^2_{r_2} - v^2_{r_1} - w^2}{2g}$, remaining. This is 0 when $v^2_{r_2} = v^2_{r_1} + w^2$.

It does not matter whether w has its maximum value at r_1, as is usual, or whether it has its maximum value at r_2, as Mr. Harris suggests, as far as the fact that it must be produced by the pump is concerned. The maximum value must be used in the equation given.

w should have its maximum value at r_1, because that is the natural place for the smallest section of the pump, and nothing is gained by shifting it outward. The actual size of the orifice at r_1, measured around the circumference of the circle and corrected for the angle at which the water enters, is, in the case of radial vanes,

$$a = 2\,\pi\,r_1\,b_1\,\frac{w}{\sqrt{w^2 + v^2_{r_1}}} = 2\,\pi\,r_1\,b_1\,\sin.\,\theta,$$

where b_1 is the width of the vanes at r_1, and θ is the angle included between the direction of the water and the direction of motion, just within the vanes at r.

Such a pump would have large friction losses at r_1 where the velocity, v_1, is suddenly produced. This can be avoided by inclining the vanes to admit the water gently; but this introduces other elements into the design.

The following equations are illustrated by reference to Fig. 36.

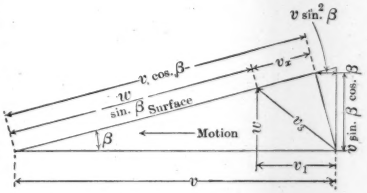

FIG. 36.

A surface is inclined at an angle, β, to the direction of its motion. Its velocity of motion is v. The component of this, normal to the surface, is v sin. β. w is the velocity of flow normal to the direction of motion of the surface. v_1 is the velocity of motion of the water parallel with the motion of the surface, however produced. The actual flow of water is represented by v_3.

$$v^2_3 = v^2_1 + w^2 = v^2\,\sin.^2\,\beta + v^2_x.$$

v_x is the velocity of the water measured parallel with the surface.

$$w = (v - v_1)\,\tan.\,\beta,$$

$$v\,\sin.\,\beta = v_1\,\sin.\,\beta + w\,\cos.\,\beta;$$

$$v_x = \frac{v_1 - v\,\sin.^2\,\beta;}{\cos.\,\beta} = v\,\cos.\,\beta - \frac{w}{\sin.\,\beta};$$

or,

$$v\,\cos.\,\beta = v_x + \frac{w}{\sin.\,\beta}.$$

Mr. Venable. If we consider two such plates, parallel to one another, with an orifice between them, the identity of this expression,

$$v \cos. \beta = v_x + \frac{w}{\sin. \beta},$$

with the expression derived for a moving orifice, $v = v_1 + v_2$, will be easily seen. The other formulas for an orifice can be readily applied to this case.

The plate, however, does not move actually against water at rest. It produces a velocity, $v \sin.^2 \beta$, in the direction of its motion.

$$h_1 = \left(v^2 \cos.^2 \beta - \frac{w^2}{\sin.^2 \beta} \right) \div 2g$$

is the pressure head at the orifice.

$$h_2 = \frac{v_3^2}{2g} = \frac{v_1^2 + w^2}{2g}$$

is the velocity head actually present between the plates, which may be converted into head by proper retardation.

$$2g\ (h_1 + h_2) = v^2 \cos.^2 \beta - \frac{w^2}{\sin.^2 \beta} + v_1^2 + w^2.$$

But, since $v_1 = v - w \cot. \beta$,

$$2g\ (h_1 + h_2) = v^2 (\cos.^2 \beta + 1) - 2\, v\, w \cot. \beta.$$

If we make $\beta = 90°$, we have again the case with radial vanes. Then :

$$\sin.^2 \beta = 1; \quad \cos. \beta = \cot. \beta = 0; \quad v_1 = v;$$

$$h_1 = -\frac{w^2}{2g}; \quad h_2 = \frac{v^2 + w^2}{2g}, \text{ and}$$

$$h_1 + h_2 = \frac{v^2}{2g},$$

all of which correspond with previous results.

If, instead of plates, such as shown in Fig. 36, there are curved plates, as shown in Fig. 37, the same velocity, v_3, may be produced gently, instead of suddenly, with the same production of useful head. The actual cross-section of the stream of water increases as the velocity parallel to the blades diminishes. The velocity of flow along the plate is $\frac{w}{\sin. \beta}$. At the entrance, this is $\frac{w_1}{\sin. \beta_1}$, and at the discharge from the blades it is $\frac{w_2}{\sin. \beta_2}$. The head developed is

FIG. 37.

$$\left[\frac{w_1^2}{\sin.^2 \beta_1} - \frac{w_2^2}{\sin.^2 \beta_2} \right] \div 2g.$$

The total head produced by the moving orifice plus the curvature of the blades is

Mr. Venable.

$$h_1 = \left[v^2 \cos.^2 \beta_1 - \frac{w^2_2}{\sin.^2 \beta_2} \right] \div 2\,g,$$

and the final velocity produced is

$$v^2_3 = (v - w_2 \cot.\ \beta_2)^2 + w^2_2.$$

There may be a series of inclined blades arranged around an axis of rotation in one of two ways. These two arrangements are illustrated in Figs. 5 and 16. In Fig. 5 the blades are arranged so that the water is intended to flow from the axis of rotation outward. In Fig. 16 the water flows in the direction of the axis of rotation. Pumps with a radial direction for w are usually called "centrifugal"; the others are "screw" or "turbine" pumps. The same fundamental formulas apply, but they must be adapted to take into account the different values of v at various points on the blade, owing to variation in r. This variation affects the two classes differently. Let ω = revolutions per second of the moving part.

The manner of converting the velocity of water after it leaves the blades is also different in the two kinds of pumps. In the "centrifugal" type, one chamber which encloses the pump collects all the discharges between the vanes and retards their velocity. In the other case, deflector plates must be used to reduce all the velocities to one direction and intensity.

In the case of the centrifugal type, the formula becomes

$$2\,g\,(h_1 + h_2) = (2\,\pi\,\omega)^2\,(r^2_2 + r^2_1 \cos.^2 \beta_1) - 4\,\pi\,\omega\,r_2\,w_2 \cot.\ \beta_2.$$

If the width of the vanes is uniform, $w_2 = \frac{r_1}{r_2}\,w_1$, and

$$2\,g\,(h_1 + h_2) = (2\,\pi\,\omega)^2\,(r^2_2 + r^2_1 \cos.^2 \beta_1) - 4\,\pi\,\omega\,r_1\,w_1 \cot.\ \beta_2.$$

It will be seen that, when r_2 is very large compared with r_1, the angle, β_2, is not very important; but that, when r_2 and r_1 are not widely different, both β_1 and β_2 are of very great importance in producing head.

The following considerations should fix the curvature of the blades.

1.—β_1 must be such as to allow the water free entrance with w normal to the direction of rotation, when the pump is operating at the conditions for which it is especially designed.

2.—β_2 must be such as to produce the required head, in connection with the other dimensions of the pump, namely, r_1, r_2, and the width of the vanes as the water passes out.

3.—The curvature from β_1 to β_2 must be such as to give the water a continuous change in velocity as it passes through, and as nearly a uniform change from w to v_3 as possible.

The pump casing should also be formed so that the velocity, v_3, will be reduced gently and uniformly to the velocity in the discharge pipe. This implies a steady enlargement of the section of the passage, measured normal to the direction of flow.

Mr. Venable. Centrifugal force is incidental to changes in velocity, and its effect is included in a consideration of the specific effect of all velocity changes.

A pump, to attain the highest efficiency consistent with economical construction, must be designed for a particular speed, head and discharge. Change in any of these elements will affect the efficiency injuriously, or will not result in as good efficiency as can be secured at the same cost with a modified design. If a pump is to operate at various heads, its design should be adapted to secure the best results at some intermediate head, so that the average efficiency will be as high as possible.

The problem of designing a pump in which w is parallel with the axis of rotation is one of shaping properly the vanes and deflector plates at various radii so as to secure a uniform value of $h_1 + h_2$ at all radii. The formulas given herein are applicable for each particular radius. It will readily be seen that sin. β_2 cannot advantageously exceed 90° in such a pump.

In a centrifugal pump it will be noticed that h_1 is positive only when

$$v \text{ cos. } \beta_1 \text{ exceeds } \frac{w_2}{\text{sin. } \beta_2}.$$

Therefore h_1 is larger the smaller $\dfrac{w_2}{\text{sin. } \beta_2}$.

Now, w_2 cot. $\beta_2 = v - v_1$,

or, $\dfrac{w_2}{\text{sin. } \beta_2} = (v - v_1)$ cos. β_2.

The motion of the blade cannot possibly produce a motion of the water in contact with it greater than v in the direction of v, or v_1 is always less than v. Therefore, if cos. β_2 is negative, either w_2 is negative, or a negative head is being created by the curvature at β_2, owing to pressure developed at other points being consumed there in producing the velocity, v_1, greater than v. That part of the blade with cos. β negative must act as a motor opposing the pump. In fact, if cos. β_2 is made sufficiently great, w will become negative, and the pump discharge water in a reverse direction from the ordinary one.

To recapitulate: All pumps of the general class under discussion rely for their production of head upon the retardation of velocity of the water relative to the walls of the passages in which it is flowing. This retardation is in two sets of passages, one of which is moving, in which the water acquires a velocity which should not exceed the velocity of the moving walls, measured in the direction of that motion, and the other set of which is fixed, and enlarges in section in order to secure head from the actual velocity of the water as it enters the fixed passages.

The sum of the head produced in these two sets of passages is the total head of the pump, which must counterbalance opposing head,

overcome friction, and create the velocity necessary to cause the water to enter between the blades at the intake. Mr. Venable.

E. T. ADAMS, Esq. (by letter).—Mr. Harris presents an exceedingly simple and sensible treatment of the theory of centrifugal pumps and fans. It is a subject regarding which, as Mr. Harris points out, very little valuable or accurate information has been published. It does not follow from this, however, as the paper leads one to infer, that nothing valuable or accurate is known regarding centrifugal pumps. It is probable that each of the principal builders of pumps of this class have in their practice anticipated many of the discoveries that Mr. Harris has made and set forth in this paper. It is certainly true that practically all the more important points presented by Mr. Harris were discovered independently in the practice of the Allis-Chalmers Company, and the writer feels the better able to express his appreciation of the merit of the discussion by Mr. Harris, because, from the practice of that company, he is able to give numerical results as a measure of the value of theories which Mr. Harris has approached entirely from a theoretical and mathematical standpoint. Mr. Adams.

The paper is an able and valuable contribution to a subject which, in the past, has not received, from our universities and their professors, the attention which its magnitude and theoretical interest justified.

ALLEN HAZEN, M. Am. Soc. C. E.—The speaker has examined this paper with a great deal of interest, and feels that it is a very valuable addition to the literature on the subject. He has not attempted to follow the mathematical discussion. Mr. Hazen.

During the past year or two he has had occasion to see quite a number of centrifugal pumps, designed and built in different shops, which departed radically from the old forms, and which were, in his judgment, better classed as turbine pumps or impulse pumps than as centrifugal pumps; and he has been much impressed with the fact that, apparently, the art of constructing these pumps has developed in certain shops to a point considerably in advance of that reflected by the literature of the subject.

Many of the matters which have already impressed him in connection with these pumps are discussed in this paper and are embodied in the designs submitted, although the designs recommended by the author differ widely in form from any pump which the speaker has seen.

The speaker believes that pumps of these advanced types, that is to say, pumps depending largely upon impulse action, and in which the friction of water moving at high velocities over the pump surfaces is systematically reduced to the lowest possible point, will play a very large part in the pumping of the future.

Among other things, the adaptability of such pumps to direct connection with high-speed rotary motors, their low cost and good effi-

Mr. Hazen. ciency will lead to their use in many places; and it is now well demonstrated that, with properly designed pumps, the old limit of economical lift of a centrifugal pump is no longer a real limit; it applied only to pumps of certain design, and when the design is adapted to the new service, it is possible to lift water to any required height with substantially the same efficiency that is obtained with very moderate lifts.

Mr. Mayer. JOSEPH MAYER, M. Am. Soc. C. E. (by letter).—This paper is very valuable in some of its suggestions for improvements, but it is marred by incorrect theories as to the vanes and the factors governing the discharge.

As regards the discharge, Q, the author seems to miss entirely some of the essential conditions governing it. The pump produces a total head, H, which is given correctly by the author. This total head is used to overcome the difference in level between the lower and the upper basins and all the resistances opposing the flow from one basin to the other, through the suction pipe, the pump and the force pipe. The loss of head in the pipes varies approximately as the square of the velocity of flow through them. The loss of head in the pump is composed of that by friction in the channels between the runner- and guide- vanes and that due to the only partial transformation into pressure of the absolute exit velocity from the runner. The first part is different for open and for enclosed runners, since it is governed in the former case partly by the absolute velocity of the water flowing along the stationary casing, and in the latter altogether by its velocity relative to the runner, which latter may be only a fraction of the former.

A runner which is in contact with water remaining in the pump causes, by the friction between it and this water, an increase in the power required to drive the pump with a given velocity, but this does not cause any loss of head in the water flowing through the pump.

If the constant peripheral velocity of the runner is u, then the total head, H, of the pump given by the author is

$$H = \frac{u^2 + u\,V \cos.\,\beta}{g}$$

If the difference in level or head between the upper and lower basins, or the lift, is H^1, then $H - H^1$ is the head available for overcoming all the resistances to the flow of the water through the pipes and the pump. All these resistances are functions of the velocity of flow; the velocity of flow, and the consequent discharge, is therefore a function of $H - H^1$; it is not proportional to u, as stated by the author, but is 0 for all values of u, as long as $H^1 = H$.

The author uses the term "efficiency of pump" in a loose manner, implying that the efficiency is the ratio $\frac{H^1}{H}$. The efficiency of a centrifugal pump, as of any other machine, is the useful work done by

it divided by the work put into it by the motor. The useful work done by the pump is the weight of water discharged multiplied by the lift, say $H^1 w$. Mr. Mayer.

The work put into the pump is $H w$ plus the work done to overcome the friction in the bearings and that between the runner and the water remaining in the pump, plus the work done in driving the leakage past the pump. If the sum of the last three items is A, then the efficiency of the pump is

$$\frac{H^1 w}{H w + A} = \frac{H^1}{H + \frac{A}{w}}$$

This, evidently, is always smaller than $\frac{H^1}{H}$, since A can never be 0.

The author is in error as regards the principles which must govern the design of the vanes. He asserts that in Fig. 20 the velocity of exit at d will be much larger than at a. The opposite is true. He neglects the fact that on account of the circular motion of the water beginning on the vertical line, $f\,b$, and the consequent increase of pressure from b toward f, due to centrifugal force, the relative velocities in the section, $b\,f$, must decrease in going from b to f, since the increase in pressure can only be produced at the expense of a decrease in velocity. The average velocity in $b\,f$, therefore, is less than the velocity, u, of the vane. Since the water originally above the level of f will nearly all pass up the vane, there will be a rise at b; that is, the vertical distance from f to the surface will be larger than $b\,f$ in the figure. During the relatively circular motion along the vane the velocity of each particle of the water (neglecting the effects of gravity and friction) will be constant, but those particles near the vane will move more slowly than those at the surface; the velocity of the particles gradually decreases and the pressure gradually increases as one passes from the surface to the vane. Before and near $a\,d$, of the figure, the same condition prevails, the total head, velocity plus pressure head, being the same from the surface to near the vane. Shortly beyond $a\,d$, where the motion is straight, the pressure is uniform through the whole cross-section, and also the velocity. The friction along the vane was here neglected; this will cause a diminution of the velocity of the particles passing at d.

The vanes of an axial-flow pump will now be considered. In such a pump each particle of water maintains during its flow through the vanes an approximately uniform distance from the axis of the pump, while, in a radial-flow pump, this distance increases. This difference in the direction of flow, relatively to the runner, entails a fundamental difference in the proper shape of the vanes.

It is generally conceded that the vanes should be shaped so that the water enters without shock, and it is perceived that the tangent

Mr. Mayer.

of the angle, Δ, of the author, between the directions of the relative entrance velocity and the velocity of the runner at the entrance should be equal to $\frac{v_1}{u_1}$, where v_1 is the absolute velocity of entrance of the water into the runner, and u_1 is the tangential velocity of the runner at the point of entrance (see Fig. 38). The elevation shows the intersections of the vanes with a cylinder, of radius, r, after development; r being the mean radial distance of the vanes from the axis of the runner. If the radial width of the vanes near the entrance is constant, and if the velocity of entrance of the water is to be constant, the first part, $a\ b$, of the vanes should be straight. If this part is not straight, but convex toward the flow, then the velocity of entrance, as the vane passes a given point, increases, while the part $a\ b$ is passing in front of that point, in proportion to the increase of the tangent of the angle of inclination of the tangent to the vane curve with the plane of rotation. This increased velocity is suddenly checked when the point of the next vane comes around. If the vane recedes far from the straight line, $a\ b$, the acceleration needed—which must extend for some distance into the suction pipe—may not be produced, and a vacuum will be created on the farther part of the back of the vane. There will certainly be a vacuum at the front of the vane, at and near the entrance. The velocity of entrance will be rapidly oscillating.

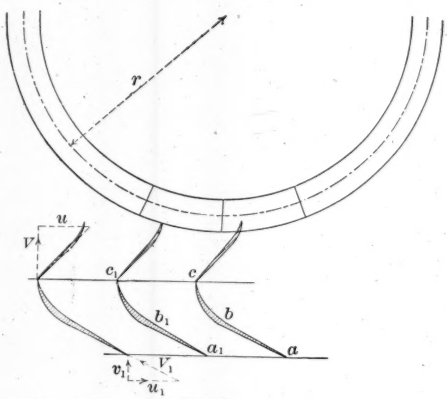

FIG. 38.

To secure absolute uniformity of the entrance velocity, the vane should be straight to the point where it begins to be covered by the following vane looking in the direction of the absolute velocity of the entering water. A small variation in the entrance velocity, however, causes little loss of head; it is, therefore, sufficient to make the vanes straight for a shorter distance, say up to the point where they are covered by the following vanes when looking at right angles to the relative entrance velocity.

The lengths of the channels in the runner, and the loss by friction in it, are thereby reduced. This argument must be modified when the vanes are of decreasing radial width near the entrance. Then a uni-

form absolute velocity at the entrance produces an increasing absolute velocity as the vane is entered. The curve of the vane, in this case, should begin at the entrance. If the rate of reduction of radial width of vanes is given, their proper curve, corresponding to a uniform velocity of entrance, can easily be constructed. The acceleration of the water, in this case, does not extend beyond the vanes, the corresponding reduction of pressure, therefore, is small, and can be calculated accurately. Mr. Mayer.

If the remainder of each vane is an arc of a circle, of radius, ρ, the water will begin to be deflected from its straight course at b b_1; it will, after this, move relative to the wheel approximately in arcs of circles of radius, ρ. Near the line, c c_1, and within the wheel, the mean relative velocity of the water is V. The centrifugal force produces an increase of pressure as one advances from c to c_1. Since the pressure in front of the vane at c_1 is greater than the pressure in the rear of the vane at c, the velocity at c_1 in front must be less than at c in the rear of the vane. Near the center of c c_1 will be the mean velocity, V, and the mean pressure, p. The absolute pressure in the rear of the vane at c cannot be less than 0. The centrifugal force along c c_1 produces a difference of pressure, p^1, per unit of area at the two points, c and c_1, where

$$p^1 = \frac{d}{g}\frac{W}{\rho}\frac{V^2}{\rho}$$

where

$d = c\ c_1$;
$W =$ weight of cubic unit of water;
$g =$ acceleration of gravity;
$\rho =$ radius of curvature;

all in the same unit.

The foregoing equation is only approximate, but sufficiently so, if the variation in velocity between c and c^1 is not very large.

One-half this pressure, p^1, will be the reduction of pressure at the back of the vane at c, below the mean pressure, p. $\frac{p^1}{2}$, therefore, must be smaller than p plus atmospheric pressure; otherwise a vacuum will be produced at c, in the rear of the vane. This gives certain limits for V and ρ and d which must be kept in view in determining these quantities.

The author seems to appreciate, in Figs. 23 and 24, the necessity of having the part of the vanes near the entrance straight, but he neglects this condition in all other vanes shown. He shows in Fig. 24 fixed guide vanes with a straight entrance. The conditions determining the shape of these vanes, however, are different from those in the runner. Either the pressure is much greater than at the entrance to the runner, or the angle between the entrance and exit of these vanes is much less than in the runner. The velocity decreases during the flow through these vanes, and the pressure increases. Consequently,

Mr. Mayer. there is much less chance for the formation of a vacuum. The curvature of these vanes may be much sharper, and may begin nearer the entrance, it can be calculated in a similar manner to that of the vanes of the runner. The vanes shown on Fig. 24, evidently, would better perform the office of turning all the outflowing water into one direction, if their straight part were shorter and their curved part were of greater radius.

The use of such fixed guide-vanes, however, can be avoided, with advantage, by introducing, at the exit from the runner, a circular channel gradually increasing in cross-section, such as shown by the author for radial-flow pumps.

Vanes of Radial-Flow Pumps.—The condition that the water should enter the pump with uniform velocity, and, therefore, without shock from the moving vanes, determines the curve of each vane as far as it is not covered by the following one when looking from the center of the pump. This curve is a spiral when the axial width of the vanes near the entrance decreases in proportion to the distance from the axis. If the axial width of the vanes decreases at a greater or smaller rate than the distance from the axis increases, then the radial velocity of the water must increase or decrease as its distance from the center increases, if a uniform velocity of entrance into the wheel is to be secured. If the rate of decrease of axial width of vanes is given by an axial section of the pump, then the proper shape of the vanes near the entrance, so as to secure a uniform entrance velocity, can easily be constructed. The part of each vane covered by the following one can be an arc of a circle meeting the circumference of the wheel at a right angle. It is not essential that the entrance velocity should be absolutely uniform. The circular part of the vanes, therefore, can begin somewhat earlier than given above, without much loss at the entrance, and with a reduction of the friction loss in the wheel. See Fig. 39. If the circular arc for the latter part of the vane is drawn tangent to the assumed end of the first part of the vane with arbitrary radius, and if the periphery of the wheel is drawn normal to this circle, then the absolute curve of the moving water near the exit from the runner can be constructed, provided the

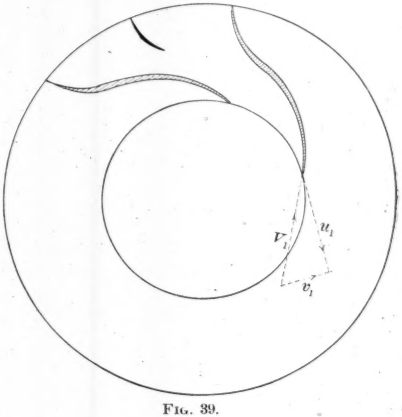

FIG. 39.

velocity of the runner, the radial width of the vanes, and the discharge are given. From this curve of the absolute motion of the water, in the runner near its exit, its radius of curvature at the exit can be determined. This corresponds to the radius, ρ, of the axial-flow pump. Mr. Mayer.

The centrifugal force between two succeeding vanes can then be calculated in a manner similar to that shown previously. Half the pressure from centrifugal force subtracted from the mean absolute pressure at the exit from the pump, calculated from the formulas, gives the absolute pressure at the back of the vanes at their outside ends. This should be more than 0. If this is not the case, then the number of vanes near the periphery of the pump should be increased, or their radius of curvature made larger. The number of vanes can often with advantage be made twice as large near the periphery as near the center. The shape here suggested for the outside part of the vanes is to some extent arbitrary. To reduce the loss of head by friction during the flow through the vanes, their radial depth should be as small as is compatible with the avoidance of a vacuum and of considerable shocks at the entrance. The best vane, therefore, will be obtained if the curvature is made as sharp as is compatible with these conditions. If this is done, the absolute pressure on the back of the vane, from the periphery to the point where the vane is not covered by the following one, will be 0. From this condition, the best curve of the vane can be constructed when the amount of flow, the axial widths of the vane, and the velocity of the wheel are given. The process, however, is complicated.

When the pressure, p, is great, and the velocity, V, is small, ρ becomes very small. In this case no vacuum need be feared, and the curvature of the latter part of the vanes can be chosen freely to suit other considerations.

If the vanes are designed correctly, which is rarely done, a pump can be constructed which will work economically with much greater speed and entrance velocity than is practicable with a badly designed pump.

Most of the designs of the author have very complicated vanes of double curvature, which can hardly be calculated and would be difficult to mould exactly to the calculated shape.

The surface of the vanes of radial-flow pumps is cylindrical, that of the vanes of axial-flow pumps is a skew surface produced by the motion of a straight line always intersecting the axis and the vane curve on the cylinder with radius, r (given previously), and always remaining perpendicular to the axis.

The Radial Depth of Vanes of Axial-Flow Pumps.—To make clear one of the most important conditions which must govern the radial depth at the exit from such vanes in an economical pump, the author's compound pump of Fig. 32 will be considered.

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The radial depth of the vanes scales 2 ins.; the head, H, produced by one set of moving vanes, varies, then, from $\frac{u_o^2}{g}$ to $\frac{u_1^2}{g}$, if u_o and u_1 are the least and greatest velocities. d_o is 12 ins.; d_1 is 16 ins., with 500 revolutions per minute;

$$u_o = 26.18 \text{ ft.};$$
$$u_1 = 34.91 \text{ ft.};$$
$$\frac{u_o^2}{g} = 21.28 \text{ ft};$$
$$\frac{u_1^2}{g} = 37.85 \text{ ft.}$$

The head corresponding to the mean diameter of 14 ins. is

$$\frac{30.54^2}{32.2} = 29 \text{ ft.}$$

The heads produced by the two sets of vanes are twice those given above, or from 42.56 to 75.70 ft. If the adverse head, outside of the pump, equal to the lift plus the losses of head in the suction and force pipes, is 50 ft., then, evidently, the water will flow backward at the inside of the vanes with a velocity that will create losses of head equal to 7.44 ft. The velocity will be small, as there will be much loss by shock where the water enters new sets of vanes. The water will flow forward at the outside of the vanes with a velocity that will create losses of head equal to 25 7 ft. The loss of head suffered by the water in flowing through the pump near the outside of the vanes will be 25.7 ft. On account of the extremely variable velocity of flow in the channels between the vanes, there will be much internal friction of the water by eddies. The direction of the vanes cannot be adapted to these various velocities, consequently there will be much shock at the entrances to the different sets of vanes. A large ratio of $\frac{d_1}{d_o}$, therefore, is very objectionable in a pump with axial outflow from the vanes; the result, inevitably, must be a low efficiency. All calculation of the lift, or amount of flow corresponding to a given velocity, u, in a pump like that here considered, must be futile; any attempt to design for it vanes that will avoid shock is equally hopeless.

The Efficiency of Centrifugal Pumps.—The writer will disregard the author's warning, at the risk of endangering the reputation of mathematics, trusting that she is strong enough to defend her reputation, and will attempt to ascertain by calculation, as far as possible, the conditions which must be complied with to secure an efficient centrifugal pump.

The efficiency of a machine is its useful work divided by its total work. The total work is the useful work plus the wasted work or the losses.

In a rotary pump the useful work is the product of the lift into the weight of water discharged. The losses are:

Mr. Mayer

First.—The friction in the bearings;

Second.—The friction between the runner and the water remaining in the pump;

Third.—The waste by leakage from the pressure to the suction side of the runner;

Fourth.—The friction of the water while flowing through the channels between the vanes;

Fifth.—The loss in the transformation into pressure of the velocity given to the water by the vanes of the runner;

Sixth.—The losses in the suction and force pipes, if these are considered a part of the pump.

The first loss is very small, especially if the pump is well balanced and driven by an electric motor. Its size depends on the efficiency of lubrication and the condition and design of the bearings.

The second loss can be avoided almost wholly by surrounding the pump with air instead of water, as suggested by the author.

The third loss can be kept low by proper design and accurate workmanship.

The first three losses affect the efficiency, but not the lift, of the pump. The third loss reduces the amount of discharge.

The laws governing the losses of head in the suction and force pipes are well known, as the velocities in them are not unusual. These losses can be calculated accurately for any given discharge.

The laws governing the losses during the flow through the vanes are not accurately known, as the velocities are generally larger than the range of the experiments, and as the channels are somewhat irregular; but the known formulas for the relation between velocity, friction, and hydraulic radius will give approximately true results. These will be a much better guide than absolute ignorance. The writer, therefore, will use Kutter's formula:

$$V = c\sqrt{RS} \qquad \ldots\ldots I$$

where

$$c = \frac{41.6 + \dfrac{0.00281}{\text{Slope.}} + \dfrac{1.811}{n}}{1 + \dfrac{\left(41.6 + \dfrac{0.00281}{\text{Slope.}}\right) n}{\sqrt{R}\ \text{(in feet)}}};$$

in which n is the coefficient of roughness, and may be taken $= 0.009$ It may be somewhat more in a new pump with rough channels, but will soon decrease with the use of the pump.

The slope in this case is large.

The formula for c, therefore, becomes approximately

$$c = \frac{243}{1 + \dfrac{0.374}{\sqrt{R}\ \text{(in feet)}}} \qquad \ldots\ldots II$$

Mr. Mayer.

From Equation *I* we obtain

$$S = \frac{V^2}{c^2 R}$$

The loss of head, h^1, in a length, l, of a conduit is

$$h^1 = S\,l = \frac{V^2\,l}{c^2\,R}$$

If $\beta = 90°$, then

$$H = \frac{u^2}{g},$$

and

$$\frac{h^1}{H} = \frac{g}{c^2}\,\frac{V^2}{u^2}\,\frac{l}{R} \qquad \text{III}$$

In this formula, V is the velocity of flow through any channel, either in runner- or guide- vanes, relative to the channel walls. $\frac{h^1}{H}$ is the fraction of the total head lost in the channel considered.

If V and R vary much in different parts, of a channel the channel should be subdivided into different parts, with nearly constant V and R in each part, and the losses of head of succeeding parts should be added.

If the relative velocity against the different walls of a channel is different, as in open runners, then the losses of head by friction against the walls with different velocity must be calculated separately and added. This can easily be done if it is considered that the friction against a part of the periphery of a channel is equal to the friction with the same velocity against the whole periphery, multiplied by the ratio between the length of the part and the length of the whole periphery.

By means of Equation *III*, the fraction, $\frac{h^1}{H}$, for the whole length of the channels between the vanes can be calculated for any proposed shape of the vanes.

Equation *II* shows that c increases slowly with the increase of R. l is the length of the channel through the vanes, and it increases with R. On account of the increase of c with R, pumps of large discharge in comparison with the lift lose a smaller fraction of the total head by friction during the flow through the runner- and guide- vanes, than pumps of small discharge compared with the lift. The formula shows that l should be as small as it can be made. This can be accomplished by making the curvature of the vanes as sharp as is compatible with the conditions previously mentioned. The shape of the channels should be such as to give a large R; that is, they should be as nearly square as other conditions allow.

If the head is large, several pumps in succession, or a compound pump, will lose less in efficiency by the friction in the channels between the vanes than a single pump of the total lift, as R will be larger in the former case.

Mr. Mayer.

The formulas given by the author and in the foregoing leave a wide range of choice for the velocity of flow through the pump. Trial designs of vanes should be made for different values of this velocity, and the consequent relative loss by friction in the channels through the vanes should be calculated by means of Equation *III*. By repeated trials, a form of vane will be found which will give nearly the smallest possible loss of head by friction in the channels between the vanes. The conditions previously given, to avoid a vacuum and a rapidly oscillating entrance velocity, should always be observed.

The loss by the only partial transformation into pressure of the absolute exit velocity from the runner may be estimated, preliminarily, at 10% of H, for a well-designed pump without sudden enlargement of the exit channels. This loss is increased by any increase in the assumed relative exit velocity, V, from the runner. This latter, therefore, should be taken somewhat smaller than would be prescribed by the smallest friction loss between the vanes.

The calculation of the pump must start with the known desired lift, H^1, and discharge, Q. The losses of head in the adopted suction and force pipes can then be calculated and added to H^1. This sum should be increased by from 20 to 33%, to obtain the theoretical head, H. The former percentage of addition should be used for pumps of large discharge, the latter for pumps of small discharge, as compared with the head. u can then be calculated, and the vanes designed for different velocities of flow through the pump. The quantity of flow through the channels between the vanes of the runner must be assumed equal to Q plus the estimated leakage past the runner.

The loss of head by friction in the channels between the different trial vanes must then be calculated and the best ones adopted. If the first guess for the losses of head in the pump was far out of the way, a new and better guess can be made and better vanes designed.

The remainder of the six losses above mentioned should then be calculated, and, where calculation fails, guessed at. This determines the efficiency of the pump and the power of the motor required. After the pump is built it should be tested. From the tests the unknown factors previously guessed at can be inferred more closely, and in future predicted more accurately. The loss about which least can be inferred from general hydraulic principles is that due to the only partial transformation of the exit velocity from the runner into head. The laws governing its size must be inferred from numerous tests with pumps actually built.

Loss of Work by Friction between the Runner and the Standing Water Surrounding It.—We have $V = c\sqrt{R\,S}$, for the relation between velocity and friction in a water pipe. From this, $S = \frac{V^2}{c^2 R}$, and from

Mr. Mayer. this is obtained the work needed to overcome the friction on a length, l, of the pipe

$$S\,l\,a\,V\,W = \frac{W}{V^2}\,\frac{a}{R}\,l\,V^3,$$

where a is the area of cross-section of the pipe; and $\frac{a}{R} = p$, the periphery of the pipe.

If we introduce p for $\frac{a}{R}$, and call the work of friction, F, we obtain

$$F = \frac{W}{c^2}\,p\,l\,V^3.$$

$p\,l$ is the area along which the friction occurs, and V^3 is the velocity of the passing water.

The standing water in the pump will gradually begin to revolve with the runner, and the relative velocity between the runner and the water will thereby decrease. If the case fits the runner closely, the velocity of the water will be half that of the runner. This will reduce the friction between the runner and the water to about one-eighth; but there will arise friction between the water and the case, which, for a close fit between the runner and the case, is about equal to the friction between the runner and the water. The power to overcome the friction between the water and the case must also come from the runner. The total work of friction, therefore, is approximately one-quarter of that between quiet water and the runner.

Different parts of the runner have different velocities, and the formula for F must be changed accordingly. The two considerations mentioned suggest the formula

$$F = \Sigma\,\frac{W}{4\,c^2}\,p\,l\,V^3 = \frac{W}{4\,c^2}\,\Sigma\,p\,l\,V^3.$$

The case here considered, however, is widely different from that of the flow of water in a pipe or an open channel. The results obtained by the formula, therefore, may not correspond with the facts. Experiments with a pump arranged for running in either water or air, and driven by an electric motor of known characteristics, would be needed to determine the proper coefficients for the formula. Experiments with a metal disk and with a cylinder revolving in water would be still more suitable for ascertaining the general law of this friction.

The distance of the case from the runner has probably little influence on the amount of work lost by this friction. If the case is far distant from the runner, a large part of the total friction will be internal friction in the water, and the friction between the water and the case will be correspondingly smaller.

The formula is here given merely as a hypothesis of the probable law of this friction. It needs confirmation by experiment, because several steps in the argument are mere surmises.

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It is hard to see what the author expects to accomplish by his proposed adjustable pump that could not be obtained in a cheaper, better, and simpler manner by a throttling valve in the force pipe. The narrowing of his exit channels from the runner will decrease the quantity of flow, as desired, but it will cause an increased loss of head in these channels. The vanes of the runner can be made quite suitable only to one ratio between velocity of flow and the velocity, u. The best efficiency, therefore, cannot be maintained for different discharges accompanying the same speed of the pump.

The introduction of these additional vanes into a part of the pump with high velocity must inevitably decrease much the obtainable maximum efficiency. The introduction of a throttling valve into a force pipe with small velocity would have little effect on the maximum efficiency of the pump, and, if partly closed, it would reduce the maximum efficiency of the pump no more than the corresponding reduction of the smaller maximum efficiency of the author's adjustable pump. If the speed of the motor can be varied, a reduction of speed will bring about a reduction of discharge with less loss of efficiency than is inevitable with the contrivance suggested by the author.

The author is correct in assuming that the field of usefulness of rotary pumps will be largely increased by an improvement in their efficiency.

Rotary pumps of high efficiency, driven by steam turbines or electric motors, may be expected to encroach largely on the present field of reciprocating pumps, for which steam turbines and electric motors cannot be used with advantage.

To sum up the writer's review of the paper: His formulas for rotary pumps, with one unimportant exception, are correct, but, alone, they are utterly inadequate as a guide for the design of an economical pump. Such a guide can only be obtained from a careful mathematical analysis of the conditions governing the efficiency of rotary pumps. Most of the defects of the paper are due to the refusal of the author to undertake such an analysis. A perfect mathematical analysis of these conditions may be impossible, but it is easy to go much further than the author did, and thereby avoid the errors into which he has fallen. Only by carrying the mathematical investigation as far as possible can the results of tests on completed pumps be so interpreted as to ascertain the complete laws governing the efficiency of rotary pumps, and only in this way can a complete guide to the correct design of rotary pumps be finally obtained.

The author's theory of the vanes, as far as it goes, is incorrect, and it does not go nearly far enough. His vanes, like those of most pumps, are largely guesswork. He neglects in his investigation

Mr. Mayer. some of the governing factors determining the discharge. His statements in regard to it are therefore in error. He neglects the inequality of head produced at the inside and outside of the discharge ring of axial-flow pumps, and, in consequence, recommends an evidently inefficient design. He suggests an adjustable pump of no value. He makes a distinction between centrifugal and impulse pumps, saying that they are based on different principles. His equations do not sustain this assertion. The essential principle of both is the same. The water is given a velocity by means of vanes pushing it along. This velocity is reduced and transformed into pressure. The proper shape of the vanes is different for radial and for axial motion of the water. This is the only essential difference between centrifugal and impulse pumps. Centrifugal force always arises when a body is forced to move in a curve. There is no good reason given why the pressure during the flow through the runner should be constant or variable in either kind of pump. The different values of this pressure may be chosen to suit other conditions. All unnecessary restrictions are harmful, because they limit the possibility of nearly satisfying all essential conditions of great efficiency.

The author also suggests compound pumps for high lifts, which, if they are properly designed, will give good results.

The merit of the paper lies mainly in a simple derivation of correct formulas, and in suggestions for methods of balancing and reducing the friction. In spite of all defects the paper is a valuable contribution to the theory of rotary pumps.

Mr. Horton. THEODORE HORTON, Assoc. M. Am. Soc. C. E. (by letter).—It is always a pleasure to find a subject, the mathematics of which might be termed burdensome, treated in a simple and practical manner, as the author has done with the subject of the centrifugal pump. The writer believes, however, with the author, that the forces acting within a centrifugal pump are so varied and intricate, and the assumptions upon which any theory must be based are in general so different from conditions in practice, that any deduction can hardly be considered other than a general guide to experimentation.

That this subject has been overlooked by previous writers upon hydromechanics, however, as stated by the author, can hardly be reconciled by one who has read the masterly paper by William Cawthorne Unwin, M. Inst. C. E., entitled "The Centrifugal Pump."*

From a comparison between the two theories advanced by these authors, it appears that, beyond the preliminary principles of the Bernoulli theorem, there seems to be little in common; but this, as intimated, is due principally to the difference in the preliminary assumptions. In Mr. Harris' analysis a radial prism of liquid upon the rotating wheel of unit area subject to purely centrifugal force is

* *Minutes of Proceedings*, Inst. C. E., Vol. LIII, 1877-78.

assumed, and an equation is deduced for the pressure at the outer radius of the wheel. Flow is next assumed to take place, and an equation is deduced for velocities of flow through the wheel, in terms of this pressure head. A final equation is deduced for the total head on the wheel, as follows : Mr. Horton.

$$H = \frac{v^2 + u \cos.}{g}$$

This equation is surely a simple one, but the assumptions are surely very far from the real conditions of flow within the pump, and, as Mr. Venable has pointed out, the equation for flow corresponding to the pressure head is that corresponding to a free discharge against no external pressure.

In Unwin's analysis, on the contrary, no conditions are assumed regarding the action of the water within the wheel, other than what take place in actual practice. Instead of assuming a prism of water of uniform area and velocity, the column of water, as it actually moves through the wheel, with its changing sections and changing radial velocities, is considered, and, in terms of these variable elements, formulas are deduced for various portions of the wheel. Thus, for the case of a pump with a spiral casing, the following equation for the total head is given :

$$H = \frac{V_o^2 - V_i^2}{2g} - \frac{u_o^2 \operatorname{cosec.}^2 \phi \quad u_i^2 \operatorname{cosec.}^2 \Theta}{2g} + \frac{V_i u_i \cot. \Theta - u_i^2 \cot.^2 \Theta}{g} + \frac{1}{g}\left[v_s (V_o - u_o \cot. \phi) - v_s^2\right] - \frac{u_i^2}{2g}$$

In this equation:

H = Theoretical lift, as measured from the inlet of the pump to the discharge from the volute;

V_i = Velocity of rotation of the wheel at the inner radius;

V_o = Velocity of rotation of the wheel at the outer radius;

u_i = Radial velocity of water at the inner radius of the wheel;

$$= \frac{Q}{2 \pi r_i d_i - n d_i t \operatorname{cosec.} \Theta}$$

u_o = Radial velocity of the water at the outer radius of the wheel;

$$= \frac{Q}{2 \pi r_o d_o - n d_o t \operatorname{cosec.} \Theta}$$

Θ = Angle which wheel vanes make with inner circumference of wheel;

ϕ = Angle which wheel vanes make with outer circumference of wheel;

V_s = Velocity of water in volute chamber;

Q = Discharge of pump per second;

r_i = Radius of inner edge of wheel;

r_o = Radius of outer edge of wheel;

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d_i = Depth of vanes at inner edge of wheel;
d_o = Depth of vanes at outer edge of wheel;
t = Thickness of vanes;
n = Number of vanes in pump disc.

This equation, at first glance, seems to be somewhat formidable, and it is reassuring, indeed, to note that in practice the third term on the right vanishes, and, unless the casing is badly designed, the fourth term may usually be neglected. In its simple form, however, the equation may be considered complete, and, in the opinion of the writer, it is a true expression for the forces which act within the pump. The concluding remarks of Mr. Unwin, though based as they were upon experience and experiments in the early stage of development of this class of pumps, lends even more confidence to the truth of his analysis, and are worthy of presentation here. He concluded as follows:

"There appears reason to expect considerable advantage from the use of a whirlpool chamber in centrifugal pumps; and it is extremely desirable that experiments should be made on pumps so constructed. If such experiments are undertaken, it is of primary importance to design the whirlpool chamber in such a way as to prevent irregular and eddying motion. If that cannot be done, guide-blades must be used. But guide-blades are not only an additional complication in the construction of the pump, but when fixed they are exactly adapted to only one discharge, and if movable, they require complicated arrangements for moving them.

"In Plate 13, sections have been shown of three types of pump of the varieties discussed in this paper. The German pump, with a spiral casing, nearly corresponds to the second type. The other two are pumps with whirlpool chambers. One of these is of a form originally designed, the author believes, by Professor James Thomson; and pumps of this kind were constructed under the author's superintendence many years ago. The other pump is the pump on which M. Decœur experimented, as mentioned above. Neither of these corresponds exactly with what, in the author's opinion, is likely to prove the best form. Such a pump should, the author believes, fulfil the following conditions:

"(1) The inner angle of the vanes should correspond to the direction of the relative motion of the water with the normal discharge.

"(2) The outer angle of the vanes should be moderately large, say from 30° to 45°. This would insure a more moderate speed of the pump, a smaller pump for a given discharge, and a more rapid increase of discharge with increase of speed.

"(3) A moderately large whirlpool chamber to utilize the energy of motion of the water discharged from the pump disc.

"(4) A suitably proportioned spiral discharge chamber.

"(5) The passage between the whirlpool and discharge chamber should be so accurately proportioned as to secure, as far as possible, a uniform flow all round from the whirl-

pool chamber into the discharge chamber. If this passage is of varying section, the regular motion in the whirlpool chamber would more or less be broken up. Mr. Horton.

"When the highest efficiency is not required, then a pump with a spiral chamber of the best proportions, as defined above, would be more compact, and should give fairly good results. In that case, probably Θ might be advantageously made somewhat smaller than the values given above; but an extremely small angle is probably disadvantageous.

"Whether these views as to the best type of pump are adopted or not, the author hopes that the formulæ for the speed and efficiency of pumps which have been obtained may be of use. It still remains to discuss the influence of friction on the efficiency and speed of the pump, and to this subject the author hopes at some future time to return. It only remains to express his sense of the great value of the experiments carried out by Mr. Parsons, the results of which have been so frequently quoted in this paper. Without the confirmation of theoretical results by the data thus furnished the author would hardly have ventured to publish this paper."

Turning now to the more practical consideration of the subject, the writer, not long ago, had an opportunity of making studies, regarding the action of centrifugal pumps, which may be of interest, as they involved some points relating to practical design. At that time he was connected with the Metropolitan Sewerage Commission, of Boston, Mass., and, under the direction of William M. Brown, M. Am. Soc. C. E., Chief Engineer of the Commission, was in charge of the official tests of the additional centrifugal pumping plants then being installed at the four pumping stations of the sewerage system.

The pumping engines at three of these stations were of the same make and capacity, but operating under different heads. Each was a triple-expansion horizontal engine driving a vertical shaft to which was directly connected the centrifugal pump. The pumps had a nominal capacity of 60 000 000 galls. per day, were relatively of large diameter and had ample water passages. As might be expected with pumps of this type, there was little trouble in attaining the required duty, which, as the writer remembers it, was about 80 000 000, a figure which compares favorably with duties obtained from pumps of the piston type. The fourth pumping engine, however, was of very different design, operating under different conditions, and since this plant was unaccepted for a long period, during which many alterations were made, it will be described more in detail.

The contract called for a pump, having a capacity of 13 000 000 galls., capable of developing 50 000 000 duty under a head of 13 ft., measured between water levels in the suction well and discharge sewer. The machine offered was a pump operating on the center of a horizontal shaft driven by a vertical, cross-compound, slide-valve engine, with cylinders at the two ends of the shaft. The pump, itself, in the writer's opinion, was well designed, the wheel, disc chamber and casing being worked out most carefully on theoretical lines, and com-

Mr. Horton. pared with many successful wheels already built. The cylinders were of a good design for engines of this class, having an adjustable cut-off on the high-pressure cylinder. The clearance was too large for good economy, but this, it was claimed, was an inherent fault due to the type of valve motion. The pump was to be operated under a suction lift of some 8 or 9 ft., and the passages leading to and from the pump were somewhat contracted.

On its first trial, the pump failed to develop even approximately the required duty, although the capacity was nearly reached. This resulted in a long series of tests and changes, which were both interesting and instructive. The first step was to establish a curve of efficiency under a constant lift. This was obtained by running efficiency tests under different rates of discharge. The rates of discharge were obtained from gauge readings in the station which registered the depths in the discharge sewer, the latter having been rated previously by means of current meter measurements. This efficiency curve showed that the maximum efficiency, and consequently the maximum duty, was attained when the rate of discharge reached 10 000 000 galls., and then dropped off, at first gradually and afterward quite rapidly, so that, when the contract capacity of 13 000 000 galls. was reached, the efficiency and duty were quite low.

This indicated that the capacity of the pump was insufficient, or that the suction and discharge passages were too small. In the hope of increasing the capacity of the pump, extensions to the blades were made by bolting on small plates of the proper thickness and curvature. This resulted in no improvement, and they were finally taken off.

The next step was to place pressure and vacuum gauges at various points on the wheel, casing, volute, and suction and discharge passages, and tests were then run under various speeds, to determine where there might be excessive losses. These tests were very useful, and soon showed that there were no undue losses within the pump casing, but some excessive losses in the suction pipe. By substituting a trumpet-shaped entrance pipe, and enlarging its diameter at the same time, and by substituting a larger curve at a right-angle bend, these losses were reduced materially.

Attention was then turned to the steam end. A calorimeter first showed that the steam contained but a fraction more than 1% of moisture, which was hardly considered serious. The cards from both cylinders were almost ideal, except during one or two tests, when the piston rod or plunger was found to leak steam. At the boiler end, the flue gases were analyzed, and it was found that under normal conditions the combustion was quite complete. The gases left the main flue at a somewhat high temperature, and this, notwithstanding that a feed-water coil was installed just back of the main damper. In the hope of reducing the temperature of the escaping flue gases, 18-in.

baffles were then introduced into the fire tubes, but with no avail, as might have been expected, since they merely retarded the velocity of the gases of combustion and, consequently, reduced the supply of heat. Mr. Horton.

Some twenty tests, in all, were made with this plant, and it seemed as though nearly every means had been tried to locate any defects and to make corresponding changes to remedy them. On the whole, the pump itself was of ample design. The passages to and from the pump were too small, and were remediable only to a degree, as a portion of the suction pipe was cast with the pump casing. The cylinders were of proper dimensions, but the clearance spaces were too large, and resulted in an extravagant waste of steam, aggravated also by high piston speed. In fact, not until, in addition to the many other changes, these cylinders were replaced by new ones having different valve motion and less clearance did this pump finally fulfill its requirements.

Among the points which impressed the writer during his studies in connection with this pump, in regard to, not only the design, but also the testing of pumps, might be mentioned the following:

1.—For high efficiency, and consequently duty, the lower the velocity should be—low not only in the pump, but in the suction and discharge passages.

2.—That, as far as the efficiency of a wheel is concerned, there may be allowed, within certain limits, some latitude as to the radii of curvature of the vanes and their angles with the inner and outer circumferences of the wheel; the result of changes in these elements being to increase or diminish the rate of speed for the point of maximum efficiency for a wheel of any given diameter.

3.—That, for a given rate of discharge, the larger the wheel, the slower the velocities and the less the losses within the wheel.

4.—That, in specifications concerning the testing of pumps, it should be stated most clearly just what constitutes the working lift and just how it should be measured; that is, whether the head is dynamic or static, and between what points in the suction or discharge passages it should be measured.

5.—In order to afford a more uniform comparison between tests of different pumping plants, it is better to measure the lift between points on the suction and discharge as near the pump as practicable.

6.—As the curves of efficiency and duty, with constant lift, and in terms of the rate of discharge, are such that the maximum points are at a rate of discharge much below the maximum capacity of the pump, it follows, that, in order to obtain high efficiencies and duties for specified rates of delivery, there will be necessarily a spare capacity beyond these rates which may be available at a relatively lower efficiency and duty.

Mr. Horton. 7.—It follows, from the previous paragraph, that in drawing specifications, especially for sewage pumps, it might be more economical to specify that the pump shall have a certain capacity according to the maximum work required, but that the required duty and efficiency shall be for a lower rate of discharge. The pump will then not be too large, and the higher efficiencies will be realized for normal working conditions.

Mr. Harris. ELMO G. HARRIS, M. Am. Soc. C. E. (by letter).—For several years the writer has devoted most of his leisure hours to studies in hydromechanics, and, if he has learned anything well, it is that it is not safe to jump at conclusions, especially if the problem involves water and air mixed.

If Mr. Venable had read the paper more carefully he probably would not have said that the conclusions were "entirely erroneous," and would not have misapplied Equation *I* at the outset. That equation applies only when the discharge is into a medium in which the pressure is the same as that in the fluid before entering the wheel—a condition specified carefully in deriving Equation *I*. Equation *VI* would have been appropriate to this case if the data had been reliable.

The writer regretted to submit the paper without a more satisfactory means of predicting the discharge of a centrifugal (or turbine) pump. An examination of the analyses will show that this depends (among other things) on the pressure, p, at the line of discharge; and this, in the present state of our knowledge, cannot be predicted accurately. Several times in the paper mention is made of the need of experimental data concerning p. In the directions for testing, this is made prominent. The difficulty of predicting the discharge was one of the writer's reasons for proposing a pump with an adjustable discharge.

Mr. Mayer states that the paper "is marred by incorrect theories as to the vanes and the factors governing the discharge." The writer looks in vain for Mr. Mayer to clear up the difficulty in predicting the discharge. He objects to the conclusions relative to the effect of depth of the stream of water on a vane acting impulsively, as illustrated by Fig. 20, and offers a demonstration of his statement that "the opposite is true." It is the writer's opinion that Mr. Mayer has erroneously assumed that Bernoulli's theorem (that pressure head + velocity head = a constant) applies to this case where energy is being imparted to the water by some exterior force every instant of its passage over the vane. His argument would lead to the conclusion that water flowing over a weir would have a greater velocity in the top film than in the bottom. He makes the same assumption in his discussion on the proper curvature of vanes (page 234), and seems to fall into a further error concerning centrifugal

Mr. Harris.

force: He uses relative velocity and the radius of a moving vane in a formula for centrifugal pressure. It is quite possible to give the curved vanes such a motion that the water will pass between them in a straight line (the absolute path), neither giving out nor receiving energy, and yet with the same V and c used in Mr. Mayer's formula. Centrifugal force developed by a fluid depends on the curve and velocity of its absolute motion. Similar mistakes can be found in textbooks on hydraulic motors.

In Mr. Mayer's discussion of the radial depths of the vanes of axial-flow pumps (page 237) he is in error in multiplying the first two heads (21.28 and 37.85) by two to get the effect of the second pair of vanes. Note that the stationary vanes intervening check the velocity, and, in the second set of moving vanes, the action in the first is repeated. Hence, the greatest difference of head is 16.57, instead of 33.14. However, this difference occurs twice. It should be noted that, theoretically, the vane could be so curved that this difference of head at the discharge line would not occur. This is evident in the formula:

$$H = \frac{u^2 + u\ V \cos.\ \beta}{g}.$$

The angle, β, gives the means of controlling H.

Mr. Mayer condemns the proposed adjustable discharge. In defence, let it be remembered that the lift varies with the square of the speed, u; evidently, then, with any regard for efficiency, no attempt to control the discharge under constant head should be made by varying the speed. The proposition to put a throttle in the discharge pipe would be correct in the common forms of pumps, in which practically all the velocity head in the discharging water is lost; but, in designing to utilize that velocity head, it will be quite different.

To illustrate: Assume the peripheral velocity, u, of the runner, in Fig. 26, to be 40 ft. per second; then the centrifugal head would be $\frac{u^2}{2\,g} = 25$, and the velocity head would be the same, giving a total theoretic head of 50 ft. If the nozzles to receive this discharge are designed properly the water will all move with velocity, u, until it passes through the throat, $e\ e$, after which it should be possible to convert most of its velocity head into pressure head. Now assume the discharge reduced to one-half, without changing u, or the area of the orifice, $e\ e$. The centrifugal head will remain $\frac{u^2}{2\,g} = 25$, as before, but the velocity through $e\ e$ will be only 20, and the pressure there cannot exceed the centrifugal head. Hence, there is now no possibility of getting a greater head than $\frac{u^2}{2\,g} + \frac{20^2}{2\,g} = 31\frac{1}{4}$. The loss would be due to the drag of the water moving with high velocity, u, in contact with that moving with low velocity. The writer is confident that the

Mr. Harris. highest development of centrifugal pumps will show the discharge to be adjustable.

Mr. Mayer's conclusions are rather sweeping condemnations of the paper. Several of his objections have been answered in such a way that evidently either he or the writer has jumped at conclusions.

To Mr. Horton's objection, that the analyses in the paper are based on questionable assumptions about the conditions inside the pump, reference is made to the "Independent Derivation of Chief Formulas" (page 175).

In closing this discussion, the writer wishes to express regret that the paper has not brought out, through the discussions, more reliable data by which the theories and formulas could be tested, and from which the corrections and coefficients could be gotten that would adapt the formulas to conditions of practice. From interviews with several of the foremost establishments making centrifugal pumps, the writer is confident that such data are in existence, and regrets that they are not available; but he cannot complain, for such experiments are costly, and manufacturers do not undertake them for the benefit of the public. There is here an opportunity for engineering schools with well-endowed hydraulic laboratories to do the public a service; and, in passing, it may be added that what has just been said applies also to the air-lift pump.

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TRANSACTIONS.

Paper No. 964.

AN EXPERIMENTAL STUDY OF THE RESISTANCES TO THE FLOW OF WATER IN PIPES.*

By Augustus V. Saph, Assoc. Am. Soc. C. E., and Ernest H. Schoder, Jun. Am. Soc. C. E.

With Discussion by Messrs. A. Flamant, Hiram F. Mills, Edgar C. Thrupp, Allen Hazen, E. G. Coker, George H. Fenkell, Gardner S. Williams, Augustus V. Saph and Ernest W. Schoder.

Before proceeding to the presentation and discussion of the experimental data upon which this paper is based, it may be of interest to describe briefly the reasons which have led the writers to make the treatment as broad as the title indicates.

As graduate students in Cornell University, the writers had been associated in experimental hydraulics for a year and a half before the investigations to be described were begun. During that time they had experienced a number of surprises in an investigation on the effect of curvature on the flow of water in pipes,† and the necessity of refraining from jumping at conclusions, with little or no experimental evidence, had impressed them more and more as the work advanced.

In 1900, Gardner S. Williams, M. Am. Soc. C. E., Engineer in Charge of the Hydraulic Laboratory of Cornell University, made

* Presented at the meeting of September 2d, 1903.
† *Transactions*, Am. Soc. C. E., Vol. XLVII, p. 295.

an investigation,* from which one of the most interesting conclusions drawn was that the power of the velocity, with which the loss of head in water-pipes varies, is dependent on the diameter of the pipe, being about 1 for very small tubes and increasing to about 2 for large pipes such as used ordinarily in the practice of the civil engineer.

For pipes below ½ in. in diameter, the value seemed to drop rapidly, while for pipes above that diameter the plotting showed a very gradual rise. However, the experimental results in the region where the most rapid change in the power seemed to occur were too few to give that feeling of entire confidence which is evident in the case of most of the commonly accepted laws of Nature. The desirability of further evidence was first pointed out to the writers by Professor Williams, and it is due to his interest in the matter, and to the true university spirit displayed by the College of Civil Engineering of Cornell University, that the writers were enabled to undertake experiments which, whatever their final aim became, had, as their primary object, the determination of the law governing the flow of water in small pipes.

The writers entered upon the investigation with the feeling that the small sizes of the pipes to be experimented upon would in no respect vitiate the results, from an engineering point of view. As will be shown later, the nature of the apparatus and the method of experimentation were to be such as to secure a high degree of accuracy in all the measurements. The writers, in their studies on the effect of curvature, had reached the conclusion that, in the manner of flow, there is practically no difference between a 2-in. and a 30-in. pipe. They expected, therefore, that much might be learned from the results of a comprehensive set of experiments on small pipes.

In addition to whatever light might be thrown upon the general subject of the flow of water in pipes, as ordinarily understood, the writers saw here an excellent opportunity to pursue their studies on the effect of the temperature of the water upon the loss of head, and hence upon the discharging capacity of pipes. The accepted idea in this matter is that the effect is negligible in pipes of commercial size. The writers had been led to question this, from their work on a 2-in. pipe,† and they saw in the experiments to be described a means of

* *Journal of the Association of Engineering Societies*, Vol. XXVI (Mar., 1901), p. 185.
† *Transactions*, Am. Soc. C. E., Vol. XLVII, p. 313.

forming an idea concerning, or even of predicting, the effect in large pipes.

Soon after beginning the experiments it was found that the power of the velocity, with which the loss of head varies, remained almost constant for the fifteen pipes of different sizes, but all of the same material (seamless-drawn brass); or, in the formula,

$$H = mV^n,$$

where H is the loss of head and V the mean velocity of the water in the pipe, the value of n was found to remain nearly constant for all the pipes. This somewhat astonishing fact appeared at first to be irreconcilable with the results of the investigation by Professor Williams; but, fortunately, the writers' experiments covered a considerable range of velocities, and it was soon found that the group of experiments on small brass and glass pipes (by Jacobson), upon the indications of which Professor Williams, with considerable justification, had placed great confidence, were all made with velocities below the "critical velocity," in which region the value of n is about 1. Hence the results were not comparable with results of experiments made under ordinary, or "above critical velocity," flow. This phenomenon will be discussed more fully later.

The fact that the value of the exponent of V was found to be almost constant for pipes ranging from $\frac{1}{10}$ in. to 2 ins. in diameter would have been sufficient of itself to warrant an investigation into the indications of reliable experiments on large, smooth pipes; for the smallest of these sizes bears the same relation to the largest as a 2-in. pipe bears to a 40-in. main (see Fig. 1, Plate VII). If this were the only argument for entering on such a study, the writers feel that they would well deserve the flood of criticism which is ever threatening those venturous persons who presume to affirm that the same laws of Nature control the flow of water in the smallest pipes in the laboratory and in the largest supply mains running over hill and dale. In this paper it is aimed to present a few additional arguments which may serve to make such an affirmation appear a little less ridiculous than heretofore.

After the experiments had progressed far enough, and several different temperatures of water had been experimented upon for each of the pipes, it became evident that there is nothing to support the notion that the effect of temperature is negligible in large, smooth

pipes. In fact, the indications were found to point to such a considerable effect on pipes of large size that the limits of error which most hydraulic engineers would assign to the best formulas in use (all of which neglect temperature effect) are exceeded several times over, owing to the temperature factor alone.

The results of the brass-pipe experiments showed such harmonious relations that the writers were led to undertake some experiments on pipes which were less ideal and more practical. For this purpose five galvanized-iron pipes, ranging from ¼ in. to 1 in., commercial sizes, were chosen. While these experiments were conducted with the same care and precision as those on the brass pipes, and while each pipe by itself gave a consistent set of results, yet, between these results for the different pipes, there was a surprising lack of harmony, if it be assumed that all pipes were of the same degree of roughness. In short, the value of n, the exponent of V, in the formula, $H = m\ V^n$, was found to vary so much for pipes nominally and apparently of the same kind that the writers saw here a consideration which might throw considerable light on the vagaries of the much-discussed, recorded pipe-experiments of modern and ancient times.

It may be well at this point to give a few reasons why the formula $H = m\ V^n$ is used. In the first place, the very fact that in the Chezy formula, $V = C\sqrt{r\,s}$, or, in the familiar textbook formula,

$$H = 4f\frac{l}{d}\frac{V^2}{2g},$$

the values of C and f are different for different velocities, shows that the power is something other than 2, if there is, indeed, any exponential law which is applicable.* And the writers venture to say that, up to the present time, there has not been evolved any theory which will assist in answering this question.

To be sure, there seems to be a more or less firmly grounded notion among some hydraulicians that the laws of gravity decree that the hydraulic slope or the loss of head must vary as the square of the velocity. The fallacy of this has been shown very clearly by Professor I. P. Church,† Assoc. Am. Soc. C. E., who emphasizes the fact that the question concerns primarily the relation between the velocity and the resistances, or "friction," and through this, as an entirely de-

* See discussion by Allen Hazen, M. Am. Soc. C. E., in *Journal of the Association of Engineering Societies*, Vol. XXVI (1901), p. 163.

† In *Engineering News*, Vol. XLVI, p. 332.

pendent matter, the relation between the velocity and the hydraulic slope. An illustration may make the point clearer: It is quite possible, by the use of compressed air, to maintain a high velocity in a very long pipe laid on an absolutely level grade with just enough head of water at the entry to keep the pipe full. Now, it is at once apparent that the law of a falling body has nothing to do with the relation existing between the pressure in the compressed-air chamber and the velocity of the water in the pipe. In fact, the force of gravity alone, in this case, would cause no flow at all.

In the absence of any theory, the only thing to do is to study the indications of experiments of known accuracy. These should embrace a wide range of velocities and several different degrees of roughness. The records of many such experiments have been before the engineer for a number of years, as have been also the records of investigations dealing with the question of exponents other than 2 for the velocity, or other than $\frac{1}{2}$ for the slope and the diameter (or the hydraulic radius). Unfortunately, the formulas with fractional exponents, which have been proposed from time to time, have been regarded generally as approximations, little, if any, closer to the truth than the standard Chezy formula, and far less easy to apply. In fact, some engineers have been led to question whether or not water flows in a pipe according to any definite determinable laws whatsoever.

Now, the writers have investigated, by the method of logarithmic plotting of the observed losses of head with the corresponding velocities, experiments in which the velocity ranges from a fraction of a foot per second up to 44 ft. per second. Their own experiments, almost eight hundred in number, and including twenty-three kinds of pipe and hose, cover a range of velocity from $\frac{1}{10}$ ft. to 22 ft. per second. In all these experiments, and especially in those of known accuracy in all measurements, the logarithmic plottings yield straight lines. The slopes of these lines differ considerably among themselves, or, in other words, the losses of head vary as various powers of the velocities,* but, whatever the slope of the line, each pipe appears to have a definite law, expressible in the form,

$$H = m\, V^n.$$

The values of m and n are constant for a given pipe in a given condition for all velocities. The "coefficient of roughness" is entirely

* *Transactions*, Am. Soc. C. E., Vol. XLIX, p. 145.

provided for by them. These appear to be scientific facts, and the writers felt that they had no choice as regards the formula to be used. As has been stated already, there is no existing theory which is applicable; but the experimental evidence is quite conclusive.

It must not be inferred, from what has been stated, that the writers are opposed to the Chezy and like formulas, or, on the other hand, that they uphold the forms with fractional exponents for practical uses; but, for purposes of investigation, it would be most irrational to start with a formula which is known to be only an approximation, especially when the exact formula is just as easy to treat. But, after such an investigation is completed, on the actual showings of the experiments, it will be easy and quite proper to look for approximate forms which may be more convenient for the engineer to use. On this point, however, more can be said, and to better purpose, after the presentation of the experimental evidence.

Enough has been stated to indicate that the scope of the investigation, as planned originally, has been enlarged considerably, and for such reasons that the writers feel that the whole subject of the flow of water in pipes may be considered open for a treatment in which the aim is to present the facts, in their relations to one another, in a thoroughly rational and comprehensive manner. It is the purpose, as far as possible, to make all plottings bird's-eye views of the whole subject, so that the experimental results may show at a glance the probable laws which too often have been obscured by misleading approximate formulas, and unfortunate attempts to make the results of the roughest kind of measurements fit these formulas. In the general plotting, many results of such rough measurements have been included. In fact, the idea has been to consider all experiments which are on record in any recognized source of information. But it is believed that the method of plotting has reduced to the greatest possible extent the question of individual judgment, and has allowed each experimental result to have its merited influence in controlling the final conclusions.

Description of Apparatus.

The general plan of the pipe system experimented upon is shown in Fig. 1. The pipes were located in the basement of Lincoln Hall, the principal building of the College of Civil Engineering, Cornell

PLAN OF PIPE SYSTEM.
(SCALE DISTORTED.)

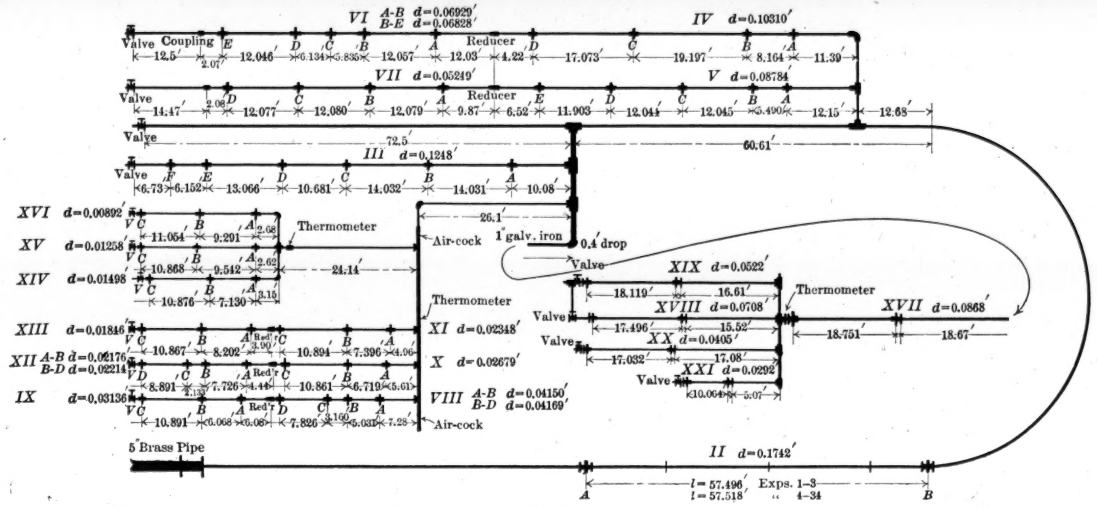

FIG. 1.

University. The system was fed through the 5 and 2-in. pipes. The former extended from an equalizing cylinder, which could be supplied either from a tank in the attic (the supply to which was controlled by a float-valve) or directly from the campus mains.* The pressures in the cylinder, under static conditions, were approximately 20 and 30 lbs., from the attic tank and the main, respectively.

All elbows, tees and auxiliary pipes are of galvanized iron except the plain iron steam-pipe header from which extend Pipes *VIII* to *XIII* and a pipe feeding Pipes *XIV*, *XV* and *XVI*. All piezometric fittings are of brass. Experimental Pipes *II* to *XVI* are of seamless-drawn brass, and Pipes *XVII* to *XXI* are of ordinary commercial, galvanized-iron stock. The fittings on all the pipes except Pipe *II*† are described herein.

Piezometric Fittings.—The individual pipe lengths of Pipes *III* to *XVI* are connected by piezometric couplings of special design. Each coupling is in two pieces which screw together in such a way that when the pipe is ready for experimentation the pressure is transmitted to an annular chamber through a continuous slit or break in the pipe $\frac{1}{100}$ in. wide. The details of the construction are shown in Fig. 2, and in Fig. 1 of Plate VII. The latter is from a photograph of piezometric couplings on Pipe *III* and Pipe *XVI*, and shows also the comparative sizes of these two pipes. The lathe work inside these couplings is practically perfect, while the differences between the inside diameters of the pipe and the coupling where the faced end of the pipe screws up against a shoulder are generally not more

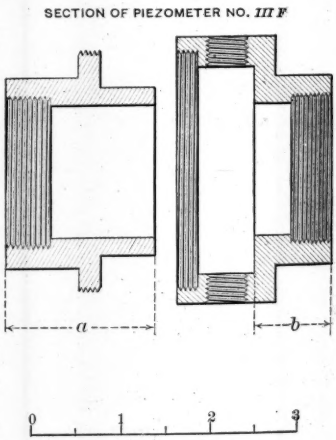

FIG. 2.

* The cylinder and the controlling valves are shown in Fig. 119, *Transactions*, Am. Soc. C. E., Vol. XLVII, p. 297. The cycloidal mouthpieces between the cylinder and the 5-in. pipe and between the latter and the 2-in. pipe are described on p. 20 of the above-mentioned volume, and also on p. 180 of the *Journal of the Association of Engineering Societies*, Vol. XXVI, 1901, while Plate 25 in the latter gives a longitudinal section of this part of the apparatus.

† The piezometer fittings on Pipe *II* have been illustrated on p. 17 and in Plate IX, Fig. 1, *Transactions*, Am. Soc. C. E., Vol. XLVII.

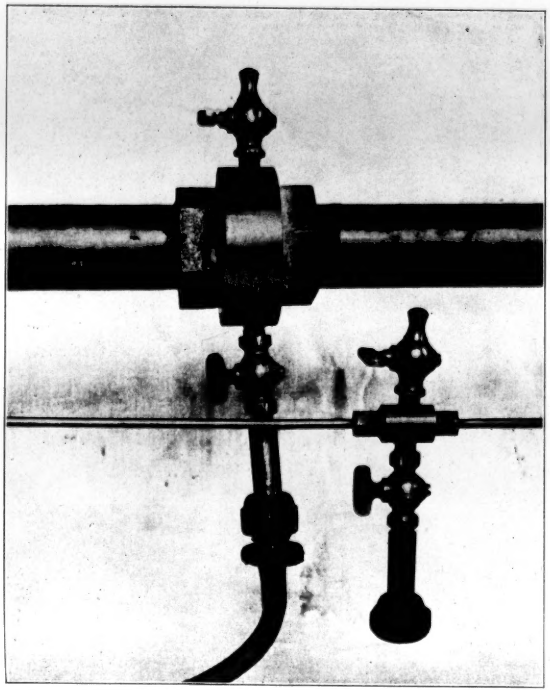

FIG. 1.—PIEZOMETER COUPLINGS. PIPES *III* AND *XVI*.

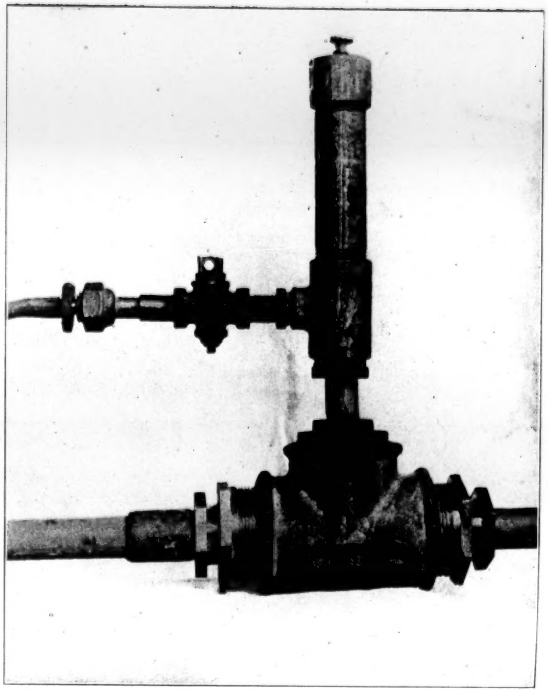

FIG. 2.—PIEZOMETER CHAMBER ON PIPE *XVIII*.

than $\frac{1}{1000}$ in., or the smallest amount which can be measured accurately with the ordinary micrometer calipers. (See Table No. 1.) Before the couplings were turned, the ends of all the pipes had been measured and the pipes matched. All threads were lathe-cut. In general, the pipes, when set up, left nothing to be desired, as far as nearly ideal conditions are concerned. The writers feel that without the excellent machine-work done by Mr. C. D. Cass, Mechanician to the Hydraulic Laboratory, the value of the experimental accuracy would have been very small. They desire to acknowledge their indebtedness to Mr. Cass for his accurate and conscientious work.

On account of the roughness of the galvanized pipes, as well as on account of the variable diameter, it was deemed best to avoid any form of joint at the point where the pressure was to be taken off. Accordingly, four $\frac{1}{8}$-in. holes, 90° apart, were drilled through the pipe walls, $3\frac{1}{2}$ ins. from the ends of each experimental length. For the two smallest pipes the holes were $\frac{3}{64}$ in. in diameter, and the distance from the ends was $2\frac{1}{2}$ ins. The drill passed through both walls of the pipe at one setting. Thus, only two of the four holes had any burr inside. This was removed carefully with a rounded file. These holes in the finished piezometer fitting communicated to a chamber formed by a large tee slipped over the pipe and provided with a pair of brass bushings at each end. Between these bushings, and on the pipe, some candle wicking soaked with tallow was wound. When the smaller bushings were screwed up, the fitting was water-tight. A photograph of one of these fittings on Pipe *XVIII* ($\frac{3}{4}$-in. galvanized iron) is shown in Fig. 2, Plate VII.* In order to avoid any error due to unequal diameters at the piezometers, all the experimental lengths were turned end for end, after the first set of experiments had been completed, and a second series was run with the reversed arrangement. In this way, also, the effect of any slight differences in the conditions of the flow at the two piezometers was eliminated. The up-stream, or entry, length of pipe was connected to the experimental length by an ordinary coupling. The ends of the pipe, however, were faced, and the threads were cut so that a butt joint was secured, avoiding the disturbing effect of the usual coupling on wrought-iron pipe where the ends do not come together.

* The air-chamber has a section like that shown on p. 17, *Transactions*, Am. Soc. C. E., Vol. XLVII.

TABLE No. 1.—Diameters (in inches) of Brass Pipes and Piezometers.

No. of Pipe.	Length No. 1.	Piezometer A.	Length No. 2.	Piezometer B.	Length No. 3.	Piezometer C.	Length No. 4.	Piezometer D.	Length No. 5.	Piezometer E.	Length No. 6.	Piezometer F.	Length No. 7.
III.	1.4976 1.4986	1.498	1.4984 1.4976	1.498	1.4986 1.4983	1.498	1.4979 1.4982	1.498	1.4978 1.4984	1.498	1.4960 1.4988	1.498	1.4988 1.4980
IV.	1.2465 1.2468	1.246	1.2376 1.2378	1.237	1.2361 1.2368	1.237	1.2378 1.2362	1.237	1.2378 1.2384				
V.	1.0693 1.0686	1.069	1.0531 1.0541	1.054	1.0543 1.0543	1.054	1.0539 1.0557	1.054	1.0539 1.0553	1.054	1.0531 1.0530		
VI.	0.8321 0.8310	0.8315	0.8313 0.8318	0.8315	0.8197 0.8201	0.819	0.8201* 0.8201	0.819	0.8191 0.8189	0.819	0.8189 0.8344		
VII.	0.6307* 0.6303	0.630	0.6297 0.6293	0.630	0.6298 0.6299	0.630	0.6303 0.6305	0.630	0.6307 0.6301				
VIII.	0.4976* 0.4982	0.498	0.4982 0.4977	0.498	0.5008 0.5006	0.500	0.5008 0.4998	0.500	0.5006 0.5008				
IX.	0.3764 0.3766	0.376	0.3763 0.3768	0.376	0.3750 0.3766	0.376	0.3766 0.3768						
X.	0.3207 0.3213	0.3215	0.3218 0.3221	0.3215	0.3215 0.3217	0.3215	0.3217 0.3213						
XI.	0.2817 0.2817	0.282	0.2813 0.2823	0.282	0.2821 0.2811	0.282	0.2811 0.2813						
XII.	0.2608 0.2613	0.261	0.2622* 0.2602	0.261	0.2652 0.2644	0.266	0.2652* 0.2654	0.266	0.2644 0.2661				
XIII.	0.2217 0.2209	0.222	0.2213 0.2220	0.222	0.2217* 0.2220	0.222	0.2217 0.2211						
XIV.	0.1807 0.1789	0.180	0.1801 0.1801	0.180	0.1800 0.1799	0.180	0.1799 0.1787						
XV.	0.1507 0.1492	0.151	0.1511 0.1522	0.151	0.1506 0.1526	0.151	0.1526 0.1514						
XVI.	0.1057 0.1081	0.107	0.1071 0.1083	0.107	0.1055 0.1073	0.107	0.1073 0.1081						

Note.—The first of the two diameters under each length is for the up-stream end in the usual arrangement. This is also the marked end, except where an asterisk denotes that the unmarked end is up stream.

Measurements of Diameters.—From the ends of all the brass and galvanized-iron pipes there were cut sample rings from $\frac{1}{8}$ to $\frac{1}{4}$ in. long. Before cutting off the rings in the lathe, the ends of the pipe were faced, the tool being started on the inside wall of the pipe, thus securing a sharp, clearly defined edge, and avoiding entirely the burr which would result if the tool were started on the outside.

Four diameters were measured on each ring. The rings from the brass pipes were placed on the bed of a dividing engine, and readings were taken as the inner edges appeared under the cross-hair of the microscope. The bed advanced 1 mm. for each turn of the screw, and one two-hundredth part of a turn could be read directly from a graduated disc on the screw. The rings from the galvanized pipes were measured with small micrometer beam-calipers reading to one-thousandth of an inch.

The means of the four measured diameters on each ring, corresponding to the mean diameters of the ends of the brass pipes, are given in Table No. 1. The numbers refer to the position of the individual pipe lengths in the experimental arrangement, the lengths being numbered consecutively, beginning at the up-street end. In a number of cases a single piece of pipe was cut into two or three lengths. Hence the question as to the uniformity of diameter can be answered by direct evidence. The following list shows what lengths were cut from the single factory lengths.

III.—No. 7 was cut from No. 6
IV.— " 5 " " " " 2
V.— " 2 " " " " 5
VI.— " 4 " " " " 3
VI.— " 6 " " " a piece, the remainder of which was not used.
VII.—No. 5 was cut from No. 1
VIII.— " 2 " " " " 1
VIII.— " 5 " " " " 4
VIII.— " 3 " " " " 4
IX.— " 1 " " " " 2
IX.— " 4 " " " " 3
X.— " 1 " " " " 2
X.— " 4 " " " " 3
XI.— " 1 " " " " 2

XI.—No. 5 was cut from No. 4
XI.— " 3 " " " " 4
XII. — " 1 " " " " 2
XIII.— " 1 " " " " 2
XIII.— " 4 " " " " 3
XIV.— " 1 " " " " 2
XIV.— " 4 " " " " 3
XV.— " 1 " " " " 2
XV.— " 4 " " " " 3
XVI.— " 1 " " " " 2
XVI.— " 4 " " " " 3

If a pipe was cut into three pieces, the shortest piece was cut off first. All pieces were cut from the unmarked end of the factory length. The unmarked end of the original pipe became the unmarked end of the first piece cut off. Hence, the marked end of a short pipe cut from a factory length represents a section in the middle of the latter.

TABLE No. 2.—DIMENSIONS OF GALVANIZED PIPES.

No. of pipe.	Length, in feet.	Diameters, in Inches.					
		Nominal.	Standard.	By Calipers.		Water-filling method.	Accepted diameter.
				Marked end.	Unmarked end.		
XVII	18.751	1-in.	1.048	1.046 1.050*	1.037 1.044	1.031 1.037	1.036 1.042†
XVIII	17.496	¾-in.	0.824	0.789 0.850	0.799 0.850	0.815 0.849	0.805 0.850†
XIX	18.119	½-in.	0.623	0.627 0.631*	0.628 0.631	0.627 0.622	0.627 0.626†
XX	17.032	⅜-in.	0.494	0.493 0.489	0.502 0.482	....	0.498 0.486†
XXI	10.064	¼-in.	0.364	0.363 0.355*	0.345 0.345	....	0.354 0.350†

NOTE.—The lengths given in the table are for the experimental sections. For other lengths see Fig. 1.

* The marked ends are also the up-stream ends in the first or unreversed arrangement, except where an asterisk denotes the opposite.

† The experimental section.

In Table No. 2 are given the mean diameters of the galvanized pipes. In addition to the end calipers, Pipes *XVII*, *XVIII* and *XIX* were measured by the water-filling method, special precautions being taken to avoid the adhesion of air to the walls of the pipe. The pipe was held vertically, and a stiff wire with a piece of cloth tied to the lower end was inserted until the cloth was near the bottom of the pipe. The pipe was then filled, and the wire was drawn up with a rotary motion. The pipe was finally filled flush, and the contained water was caught and weighed to the nearest $\frac{1}{100}$ lb. Each pipe length was filled three times. Allowance was made for the small quantity of water which remained adhering to the inside walls of the pipe, a determination having been made.

Measurements of Lengths.—All lengths were measured with an accurate steel tape, and were checked by independent measurements. They are given in Fig. 1 and in Table No. 3. For the experimental sections, they are, in all cases, between the piezometric slits for the brass pipes, and between the piezometric holes for the galvanized pipes.

Methods of Experimentation.—All pipes were laid straight, and, where necessary, intermediate supports were used between the hangers on which the whole pipe system was supported. On account of their very small sizes, Pipes *XIV*, *XV* and *XVI* were laid on a board, and held straight by tacks. The hangers were levelled up carefully.

As shown in Fig. 1, there are fourteen valves to control the flow in the various experimental lines, several of which have two different sizes of pipes. It will be noticed that there is in all cases a considerable length of pipe up stream from the first piezometer, or Piezometer *A*. In fact, the writers had agreed that it is desirable to have an entry length of approximately two hundred diameters, in order to allow the flow to become normal. While this allowance is ample, it does not appear to be excessive. A length of one hundred diameters is, in the writers' opinion, hardly enough to allow the disturbances due to a curve at entry to die down fully.*

In order to get as large a range of velocities as possible, the down-stream sections of Pipes *VIII* to *XVI* were removed and replaced again from time to time. On several occasions Pipes *VI*, *VII*, *IX*, *XII* and *XIII* were entirely removed, in order to increase the discharge of the larger pipes ordinarily feeding into them.

**Transactions*, Am. Soc. C. E., Vol. XLVII, p. 317.

TABLE No. 3.—Brass Pipe—Lengths of Sections and Diameters.—See Fig. 1.

Pipe.	Section.	Length, in feet.	Accepted diameter, in feet.	Pipe.	Section.	Length, in feet.	Accepted diameter, in feet.
II.	*A*-*B*	57.496	0.1742	*IX.*—1	Reducer-*A*	6.08	0.08187
		57.518		2	*A*-*B*	6.068	
III.—1	Entry-*A*	10.08	0.12484	3	*B*-*C*	10.891	
2	*A*-*B*	14.031		4	*C*-Valve.	1.02	
3	*B*-*C*	14.032		*X.*—1	Entry-*A*	5.61	0.09679
4	*C*-*D*	10.681		2	*A*-*B*	6.719	
5	*D*-*E*	13.066		3	*B*-*C*	10.861	
6	*E*-*F*	6.152		4	*C*-Reducer.	1.075	
7	*F*-Valve.	6.73		*XI.*—1	Entry-*A*	4.96	0.02347
IV.—1	Entry-*A*	11.39	0.10887	2	*A*-*B*	7.396	
2	*A*-*B*	8.164	0.10311	3	*B*-*C*	10.894	
3	*B*-*C*	19.197		4	*C*-Reducer.	1.06	
4	*C*-*D*	17.073		*XII.*—1	Reducer-*A*	4.44	0.02176
5	*D*-Valve.	4.22		2	*A*-*B*	7.723	
V.—1	Entry-*A*	12.15	0.08908	3	*B*-*C*	2.133	0.02209
2	*A*-*B*	5.490	0.08785	4	*C*-*D*	8.891	
3	*B*-*C*	12.045		5	*D*-Valve.	1.04	
4	*C*-*D*	12.044		*XIII.*—1	Reducer-*A*	3.90	0.01846
5	*D*-*E*	11.903		2	*A*-*B*	8.202	
6	*E*-Reducer.	6.52		3	*B*-*C*	10.867	
VI.—1	Reducer-*A*	12.03	0.06930	4	*C*-Valve.	1.02	
2	*A*-*B*	12.057		*XIV.*—1	Entry-*A*	3.15	0.01498
3	*B*-*C*	5.835	0.06831	2	*A*-*B*	7.180	
4	*C*-*D*	6.134		3	*B*-*C*	10.876	
5	*D*-*E*	12.046		4	*C*-Valve.	1.02	
6	*E*-Coupling.	2.07	0.06889	*XV.*—1	Entry-*A*	2.62	0.01961
VII.—1	Reducer-*A*	9.87	0.05251	2	*A*-*B*	9.542	
2	*A*-*B*	12.079		3	*B*-*C*	10.868	
3	*B*-*C*	12.080		4	*C*-Valve.	1.02	
4	*C*-*D*	12.077		*XVI.*—1	Entry-*A*	2.68	0.00893
5	*D*-Enlargement to valve.	2.08		2	*A*-*B*	9.291	
		14.47		3	*B*-*C*	11.054	
VIII.—1	Entry-*A*	7.28	0.04149	4	*C*-Valve.	1.02	
2	*A*-*B*	5.031		.....	.....	.....	.....
3	*B*-*C*	3.160	0.04172	.....	.....	.....	.....
4	*C*-*D*	7.826		.....	.....	.....	.....
5	*D*-Reducer.	1.060		.....	.....	.....	.....

The differences of pressure between the piezometers were measured by either mercury or water gauges, of the differential type, the pressures being transmitted from the piezometers to the gauge columns by heavy, three-ply, cotton-insertion, rubber tubing. The scales of these gauges are divided into double centimeters and tenths, the hundredths being estimated. All gauges were supplied with riders to facilitate accurate reading of the menisci. The mercury gauges had three columns, and the water gauges had either two or three columns. Any of the gauges could be used as two-column gauges, if desired.

The water was discharged for measured intervals of time into a tank on one of three platform scales. One of these scales has a capacity

of 2 500 lbs., and is accurate to the nearest pound. Another has a capacity of 350 lbs., and is accurate to $\frac{1}{10}$ lb. The third is sensitive to $\frac{1}{100}$ lb. for weights not exceeding 50 lbs. The two smaller scales were calibrated with standard weights in the possession of the College of Civil Engineering. The largest was tested by comparison.

Thus, it was possible to secure the desired accuracy for the measurement of the discharge of the smallest of the pipes as well as the largest. In general, the attempt was made to secure an average accuracy of 1 in 500 in all measurements, whether of weight, time or gauge differences. For the highest velocities, a much greater accuracy has been obtained, while, for some of the very lowest velocities, the weight or gauge difference was so small as to render the result accurate within only about $\frac{1}{2}$ of 1%, or, very rarely, within only 1%; but, for the vast majority of the experiments, it is believed that the attempted accuracy has been secured.

For many of the first experiments, the time was taken from a record sheet of a chronograph connected to the diverting arrangements and to the standard clock of the Astronomical Laboratory. It was found that the error in using an ordinary well-regulated watch was in general only a small fraction of a second. In fact, by using a small reading glass, the $\frac{1}{5}$-second beats can be seen quite distinctly on the second hand of the ordinary watch. Hence, for all the later runs, the time was taken with an ordinary watch, and it is believed that the time element is well within the attempted limit of accuracy.

During the time in which the flow was being measured, the gauge was read from six to twenty times. When the pressure was taken from the attic tank the columns were quite steady, but when connected to the campus mains there was considerable vibration, yet not enough to impair greatly the accuracy of the readings. The main pressure was used, generally, only for the highest velocities, and hence the greatest gauge differences. The gauges were tested under no-flow conditions before and after, and frequently during, every series of runs. In general, no trouble was experienced. The columns came quickly to a common level. This condition of affairs the writers attribute to the complete control of the apparatus, the excellence of hose and gauge connections, and the great care taken to free the whole pipe system from air before starting any experiment. The temperature of the water for the first experiments was taken in the tank after the runs.

For these cases the temperature of the water was within 1 or 2° Fahr., of the temperature of the room. For the later experiments on Pipes *VIII* to *XVI*, and for all the experiments on Pipes *XVII* to *XXI* (of galvanized iron), the temperature was taken with a thermometer inserted in a stuffing-box, and with its bulb well within the pipe. The positions of these thermometer stuffing-boxes are shown in Fig. 1. The temperature was taken at the beginning and at the end of each experiment, and the mean value used, the differences being small. For Pipes *II* to *VII* it was found that the temperature in the pipes was the same as that in the tank after filling, within less than half a degree. Hence, the latter was taken, except at low velocities, when a small jar full of the water was caught just before and after each run and the temperature was read immediately.

Methods of Reduction.—In reducing weights to velocities the weights per cubic foot of distilled water at the observed temperature were taken from a table by the late Hamilton Smith, Jr., M. Am. Soc. C. E. These values are known to be correct within $\frac{1}{10}$ of 1% for the water used in the experiments.

In reducing observed heads on the differential water gauges a correction is necessary on account of the weight of a column of air of a height equal to the observed difference and under an average pressure of, approximately, $1\frac{1}{2}$ atmospheres. This amounts to a subtraction of approximately one five-hundredth of the observed differences. There is another correction on account of the difference between the temperature of the water in the gauge and that in the pipe, but this is very small, and is an uncertain amount, varying from nothing to a possible subtraction of one-six-hundredth of the observed difference. The correction actually applied, to cover these two items, is a subtraction of one five-hundredth of the observed difference for temperatures in the pipe below 60° Fahr.

For reducing observations on the mercury gauges, the "water equivalent" was obtained by direct comparison. A line of levels was run, and the elevation of the top of the attic tank above the zeros of the gauges was determined accurately. One observer measured the level of the surface of the water in the tank, while another took readings on the gauges in the basement. Eight determinations were made, with as many different heights of the water surface. The "water equivalent" was found to be 13.57 for mercury and water at 71° Fahr.

The eight determinations agreed within $\frac{1}{10}$ of 1 per cent. The weight of a column of air from the gauge to the attic was included in the calculation. Since the temperature of the room, and hence the specific gravity of the mercury in the gauges, was practically constant during the course of the experiments, the observed differences were corrected according to the specific gravity of the water flowing in the pipe. Thus all heads are reduced to water at the temperature of that in the pipe.

All reductions of weights to velocities, and of observed gauge differences to loss of head, in feet of water per 100 ft. of pipe length, have been checked by independent calculations. In addition, all results have been subjected to the test of two different methods of plotting, and any apparently wild cases have been recalculated. The writers have been pleased to find that the efforts to maintain a high degree of accuracy in all measurements have borne good fruit in the resulting indications.

Table No. 4 contains the data for the brass pipes, and Table No. 5 the data for the galvanized pipes.

Brass Pipe *III.*

30	*B–D*	2.70	*D–F*	2.69	70.2°	3.095	All heads for Pipe *III* were read on water gauges except in Experiment 194 and Experiments 308 and 309 for Sections *B–D* and *D–F* together. One experiment (309*b*.) read on water gauge.
31	"	1.96	"	1.96	70	2.576	
32	"	1.29	"	1.30	69.8	2.029	
33	"	0.709	"	0.716	69.8	1.431	
34	"	0.438	"	0.423	70	1.067	
35	"	16.04	*E–F*	16.05	68.8	8.473	
36	"	11.58	"	11.58	69.1	7.060	
37	"	8.40	"	8.34	69.1	5.887	
38	"	5.96	"	5.92	69.2	4.853	
39	"	3.47	"	3.41	69.1	3.568	
202	.....	.....	*B–F*	0.070	71.8	0.383	
203	.....	.....	"	0.201	71.8	0.870	
204	.....	.....	"	0.140	71.7	0.571	
194	*B–D*	17.74	*D–F*	17.70	68.8	8.994	
308	"	18.33	"	18.34	62.0	9.037	
309	"	15.70	"	15.88	62.5	8.280	
309*b*	"	15.89	.....	.....	62.5	8.280	
310	"	11.77	.....	.....	62.1	7.013	
311	"	8.77	.....	.....	62.1	5.947	
312	"	5.08	.....	.....	62.1	4.361	
313	"	2.25	.....	.....	62.2	2.744	
325	"	16.06	.....	.....	56.0	8.261	
326	"	13.40	.....	.....	55.6	7.443	
327	"	9.53	.....	.....	55.5	6.147	
328	"	6.19	.....	.....	55.5	4.814	
329	"	3.50	.....	.....	55.6	3.475	
407	"	16.78	.....	.....	46.0	8.265	
408	"	13.44	.....	.....	42.2	7.239	
409	"	9.28	.....	.....	41.1	5.864	Temp. Air, 76°. Temp. for Experiments 407 to 412 read in tank.
410	"	5.65	.....	.....	41.3	4.420	
411	"	2.69	.....	.....	42.0	2.893	
412	"	16.96	.....	.....	41.0	8.238	
544	"	16.96	.....	.....	36.0	8.098	
545	"	13.53	.....	.....	36.0	7.160	
546	"	10.26	.....	.....	35.6	6.109	
547	"	6.49	.....	.....	36.0	4.708	
548	"	3.18	.....	.....	36.9	3.128	
549	"	1.12	.....	.....	37.6	1.713	
550	*B–C*	19.36	.....	.....	35.5	8.738	

TABLE No. 4.

Brass Pipe *II.*

No. of Exp't.	Section.	Loss of head, in water. Feet per 100.	Section.	Loss of head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
1				4.155	36.0°	4.591	Length of section in Experiments 1 to 3 was 57.496 ft. For Experiments 4 to 34, and 714 to 717 length was 57.518 ft.
2				2.724	37.4	3.636	
3				1.342	36.0	2.412	
4				4.746	37.0	4.966	
5				3.637	36.0	4.290	
6				4.440	42.9	4.854	
7				3.963	41.0	4.550	
8				2.813	41.5	3.737	
9				1.423	41.9	2.533	
10				0.437	42.0	1.281	
11				4.377	49.5	4.893	
12				3.737	49.4	4.473	
13				2.605	49.6	3.651	
14				1.545	49.6	2.713	
15				0.376	50.4	1.202	
16				4.281	55.2	4.889	
17				3.565	55.5	4.413	
18				2.662	55.5	3.734	
19				0.952	55.5	2.076	
20				0.285	....	1.035	
21				4.292	56.1	4.902	
22				2.711	58.5	3.774	
23				1.770	56.0	2.954	
24				0.861	56.0	1.959	
25				3.451	70	4.434	
26				5.341	69.8	5.701	
27				2.811	69.8	3.946	
28				1.604	69.8	2.886	
29				0.892	69.8	2.063	
30				0.562	70.2	1.590	
31				0.410	70	1.323	
32				0.273	69.8	1.043	
33				0.148	69.8	0.735	
34				0.095	70	0.548	
714				5.989	42.4	5.796	All heads for Pipe *II* were read on water gauges.
715				1.742	39.0	2.830	
716				0.812	38.5	1.825	
717				2.895	37.0	3.746	

Brass Pipe *V.*

48	*B–D*	4.179	*D–E*	4.146	70.3°	3.087	†Heads read on mercury gauges.
49	"	3.721	"	3.699	70.1	2.887	
50	"	2.713	"	2.707	70.1	2.415	
51	"	2.275	"	2.288	70	2.179	
52	"	1.896	"	1.885	70	1.959	
53	"	1.137	"	1.119	70	1.488	
54	"	0.527	"	0.546	70	0.942	
55	"	0.185	"	0.182	70	0.517	
209			*B–E*	0.312	71.6	0.751	
210			"	0.250	71.6	0.672	
211			"	0.123	71.6	0.447	
212			"	0.085	71.6	0.359	
263†	*B–D*	25.20	*D–E*	25.22	70.8	8.609	
264†	"	17.94	"	17.80	70.5	7.099	
265†	"	13.56	"	13.44	70.6	6.039	Temp., Air 71°.
266†	"	9.28	"	9.14	70.6	4.864	
299†	"	25.86	"	25.68	61.4	8.535	
300†	"	18.26	"	18.20	61.6	7.044	
301†	"	11.08	"	11.01	61.6	5.285	
302†	"	3.90	"	3.81	61.6	2.896	
347	*D–E*	18.67			54.5	7.030	
348	"	13.72			54.5	5.926	
349	"	8.88			54.5	4.624	
350	"	4.01			54.5	2.927	
351	"	24.67			55.0	8.208	
725	*C–D*	19.87			44	7.054	
726	"	11.56			42.1	5.133	
727	"	28.49			37.2	8.503	
728	"	21.36			36.6	7.200	
729	*D–E*	20.17			40	7.016	
730	"	12.73			39	5.395	
731	"	5.49			39.1	3.345	
732	"	27.33			35.8	8.225	

TABLE No. 4—(*Continued*).

BRASS PIPE *IV*.

No. of Exp't.	Section.	Loss of head, in water. Feet per 100.	Section.	Loss of head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
40	.....	.....	*B–D*	5.61	69.0°	4.097	† Heads were measured in mercury.
42	*B–C*	4.06	*C–D*	4.06	69.1	3.384	
43	"	2.88	"	2.88	69.0	2.794	
44	"	2.03	"	2.04	69.0	2.279	
45	"	1.39	"	1.40	69.0	1.813	
46	"	0.52	"	0.52	69.0	1.019	
47	"	0.17	"	0.19	69.1	0.549	
205	.....	.....	*B–D*	0.145	72.2	0.506	
206	.....	.....	"	0.109	72.1	0.429	
207	.....	.....	"	0.255	72.0	0.695	
208	.....	.....	"	0.371	71.7	0.867	
259†	*B–C*	15.03	*C–D*	14.92	70.1	7.365(?)	Evidently a mistake of 5 seconds in time. V=7.244 therefore used.
260†	"	10.91	"	10.77	73.2	5.975	
261†	"	8.12	"	7.92	72.8	5.052	
262†	"	10.96	"	10.82	71.0	5.994	
303†	"	15.41	"	15.54	62.0	7.135	
304†	"	11.11	"	11.34	62.2	5.959	
305†	"	7.73	"	7.92	62.2	4.889	
306†	"	4.63	"	4.73	62.1	3.637	
307†	"	1.89	"	1.93	62.1	2.154	
303	"	15.43	.....	.....	62.0	7.135	
304	"	11.19	.....	.....	62.2	5.959	
305	"	7.85	.....	.....	62.2	4.889	
306	"	4.67	.....	.....	62.1	3.637	
307	"	1.89	.....	.....	62.1	2.154	
342	"	11.44	.....	.....	54.9	5.946	
343	"	8.30	.....	.....	54.7	4.960	
344	"	5.45	.....	.....	54.6	3.890	
345	"	2.39	.....	.....	54.5	2.427	
346	"	14.39	.....	.....	54.7	6.771	
718	"	12.11	.....	.....	46.5	5.977	
719	"	8.00	.....	.....	43.2	4.698	
720	"	2.82	.....	.....	43.0	2.584	
721	"	14.53	.....	.....	37.2	6.516	
722	"	9.97	.....	.....	37.0	5.269	
723	"	4.99	.....	.....	38.0	3.542	
724	"	2.12	.....	.....	38.5	2.167	

Brass Pipe *VII.*

50	*B–C*	31.09			70.1°	6.762	† Heads read in mercury.
51	"	26.04			70	6.101	
52	"	21.61			70	5.486	
53	"	12.61			70	4.028	
54	"	6.067			70	2.637	
55	"	2.140			70	1.447	
56	"	0.652			70.7	0.728	
195†	*A–C*	77.88	*C–D*	78.10	69.3	11.372	
196†	"	60.95	"	61.24	69.8	9.889	
197†	"	46.97	"	47.11	69.7	8.560	
198†	"	33.18	"	33.05	70.9	7.001	
199	*A–D*	0.440			71.3	0.648	
200	"	0.187			71.3	0.471	
201	"	0.087			71.8	0.218	
380†	*A–C*	78.29	*C–D*	77.04	72.0	11.375	After Experiment 201 Pipe *VII* was taken out and was allowed to remain empty. When set up again, the circulation through the piezometers was poor. Upon investigation, iron rust was found in the slits. These were easily cleaned.
381†	"	79.35	"	78.45	47.5	10.940	
382†	"	67.97	"	67.41	48.0	10.056	
383†	"	52.77	"	52.37	56.1	8.836	
384†	"	41.24	"	40.77	54.6	7.635	
385†	"	31.52	"	30.95	53.2	6.525	
386†	"	21.16	"	21.13	52.5	5.208	
387†	"	10.70	"	10.77	53.0	3.521	
388	"	0.342	"	0.348	85.8	0.538	
389	"	0.834	"	0.831	84.0	0.877	Temp., Air 76° in 383 to 387.
390	"	1.524	"	1.521	81.0	1.230	
391	"	1.203	"	1.206	81.0	1.075	
392	"	2.059	"	2.052	81.0	1.609	
393	"	3.316	"	3.282	77.0	1.881	
469†	"	79.38	"	78.10	40.8	10.686	Air, 71.6°.
470†	"	53.83	"	52.48	47.5	8.708	
471†	"	42.38	"	41.57	46.1	7.542	
472†	"	32.77	"	32.37	45.4	6.512	
473†	"	20.37	"	20.17	46.0	4.949	
474†	"	10.43	"	10.36	46.6	3.361	Air, 64°.
475†	"	81.76	"	80.35	38.2	10.745	
476†	"	67.69	"	66.31	37.8	9.711	
477†	"	49.12	"	48.45	37.5	8.098	
478†	"	35.33	"	34.89	37.3	6.680	

TABLE No. 4—(*Continued*).

BRASS PIPE *VI.*

No. of Exp't.	Section.	Loss of head, in water. Feet per 100.	Section.	Loss of head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
41	*C–D*	40.15			69.0°	9.316	† Heads read in mercury.
42	"	28.93			69.1	7.712	
43	"	20.47			69.0	6.368	
44	"	14.38			69.0	5.194	
45	"	9.84			69.0	4.133	
46	"	3.66			69.0	2.922	
47	"	1.28			69.1	1.252	
180†	*A–D*	56.22	*D–E*	53.18	69.3	11.477 (For *D–E*)	11.146
181†	"	43.48	"	40.86	69.3	9.933 (For *D–E*)	9.647
182†	"	30.93	"	29.23	70.3	8.212 (For *D–E*)	7.975 Velocities in *A–B*.
183†	"	21.42	"	20.40	70.3	6.668 (For *D–E*)	6.476
184†	*B–C*	10.54	"	9.94	70.6	4.445 (For *D–E*)	4.317
185	"	59.97			69.3	11.477	
186	"	46.28			69.3	9.933	
187	"	32.84			70.3	8.212	
188	"	22.72			70.3	6.668	
189	"	11.12			70.6	4.445	After Experiment 193, Pipe *VI* was taken down and remained empty for a time. Upon setting again, the circulation through the piezometers was poor. Iron rust was found in the slits. These were easily cleaned.
190	*C–E*	1.56			70.9	1.501	
191	"	1.01			70.9	1.172	
192	"	0.722			71.1	0.962	
193	"	0.433			70.0	0.716	
399	*A–B*	30.11			51.5	7.705	
400	"	24.18			48.0	6.755	
401	"	16.71			47.0	5.470	
402	"	8.90			46.4	3.803	
403	"	1.58			46.6	1.404	
404	"	4.42			47.0	2.448	Air, 74°.
405	"	29.98			44.0	7.590	
406	"	13.19			44.0	4.720	
551†	*C–E*	58.40			36.3	11.055	
552†	"	45.72			36.2	9.638	
553†	"	34.72			37	8.223	
554†	"	24.99			37	6.841	
555†	"	11.54			37.6	4.352	
709†	*C–E*	38.60			79	9.382	
710†	"	39.72			53	9.114	
711†	"	56.74			48.7	11.125	
712†	"	58.90			38	11.131	
713†	"	27.25			45.1	7.270	

64	*A–B*	73.36			69.1°	7.868 (?)	† Heads read in mercury.
65	"	59.15			69.5	6.777	* Columns vibrating badly.
66	"	39.29			69.6	5.371	
67	"	27.24			69.9	4.356	In Experiment 64 there was evidently a mistake of 5 lbs. in the weight. This was checked by Experiment 70. Velocity should be 7.652.
68	"	15.06			70.1	3.119	
69	"	6.38			70.5	1.928	
70	"	73.05			71.0	7.631	
176†	"	172.6	*B–C*	172.2	67.5	12.28	
177†	"	117.5	"	117.7	67.5	9.884	
178†	"	68.35	"	68.29	69.0	7.286	
179†	"	40.76	"	41.04	69.3	5.334	
275†	"	173.1	"	173.7	66.1	12.31	
276†	"	115.6	"	115.8	65.0	9.758	
277†	"	79.04	"	78.86	65.0	7.855	
278†	"	47.00	"	47.00	65.0	5.820	
283	"	22.24			66.1	3.882	
284	"	14.08			67.0	2.903	
285	"	7.375			67.0	2.011	
286	"	4.89	*B–C*	4.86	68.0	1.587	
287	"	3.57	"	3.53	68.2	1.343	
288	"	1.71*	"	1.59*	68.4	1.051	
289	"	0.95	"	0.96	68.6	0.849	
290	"	0.58	"	0.60	68.8	0.541	
291	"	0.31	"	0.33	69.0	0.291	
292	"	0.12	"	0.13	69.0	0.111	
338†	"	169.9	"	170.7	55.0	11.91	Air, 67°.
339†	"	130.4	"	131.3	55.5	10.27	
340†	"	78.4	"	79.1	56.0	7.685	
341†	"	54.9	"	55.2	56.3	6.261	
510†	"	118.8	"	120.3	42.8	9.450	
511†	"	75.1	"	76.1	42.1	7.229	
512†	"	41.8	"	42.2	45.0	5.201	
513†	"	177.7	"	180.3	46.5	12.03	
514†	"	168.6	"	171.7	37.7	11.415	
515†	"	137.0	"	138.1	37.6	10.101	
516†	"	110.8	"	112.0	37.3	8.951	
517†	"	86.2	"	87.2	37.5	7.751	
518†	"	55.6	"	56.0	37.8	6.015	Air, 68.5°.
519†	"	27.3	"	27.5	37.9	4.001	

TABLE No. 4—(*Continued*).

Brass Pipe *VIII.*

No. of Exp't.	Section.	Loss of head, in water. Feet per 100.	Section	Loss of head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
57	*C–D*	49.55	.....	.....	69.4°	7.443	† Heads read in mercury.
58	"	40.30	.....	.....	69.4	6.622	* Columns vibrating badly.
59	"	32.64	*A–B*	33.49	69.4	5.833	5.895
60	"	20.59	"	21.25	69.4	4.501	4.549
61	"	11.96*	"	12.31*	69.4	3.300	3.335 Velocities for *A–B*.
62	"	4.40	"	4.54	69.4	1.850	1.870
63	"	1.38	"	1.50	69.4	0.947	0.957
172	*B–C*	43.96	.....	.....	67.5	6.941	
173	"	29.05	.....	.....	67.5	5.584	
174	"	17.63	.....	.....	69.0	4.117	
175	"	10.51	.....	.....	69.3	3.013	
168†	*A–B*	45.23	*B–D*	44.05	67.5	7.015	
169†	"	30.81	"	30.09	67.5	5.644	Velocities for *B–D* same as for *B–C*. Experiments 172 to 175.
170†	"	17.58	"	17.49	69.0	4.161	
171†	"	10.82	"	10.58	69.3	3.046	
279†	*C–D*	44.86	.....	.....	66.1	6.956	
280†	"	29.91	.....	.....	65.0	5.513	
281†	"	20.85	.....	.....	65.0	4.439	
282†	"	12.43	.....	.....	65.0	3.288	
379†	"	44.2	.....	.....	53.0	6.691	
394	"	30.66	.....	.....	51.5	5.389	
395	"	22.78	.....	.....	52.2	4.553	
396	"	15.07	.....	.....	52.2	3.584	Air, 67°.
397	"	6.18	.....	.....	53.3	2.150	
398	"	6.55	.....	.....	54.6	2.216	
556†	*A–B*	183.1	.....	.....	71	14.97	
557†	"	194.9	.....	.....	37	14.545	
558†	"	160.7	.....	.....	36.7	13.03	
559†	"	134.3	.....	.....	36	11.77	
560†	"	103.4	.....	.....	36	10.15	
561†	"	81.3	.....	.....	36.1	8.791	Air, 66°.
562†	"	58.6	.....	.....	36.1	7.370	
563†	"	38.5	.....	.....	36.3	5.770	
564†	"	23.4	.....	.....	36.5	4.332	
565†	"	134.6	.....	.....	39.2	11.87	
566†	"	114.6	.....	.....	48.8	10.99	
567†	"	80.8	.....	.....	48.4	9.063	
568†	"	54.0	.....	.....	49.0	7.252	
569†	"	26.5	.....	.....	49.2	4.824	Air, 66°.

Brass Pipe *XI.*

80	*A–B*	43.99			69.0°	4.567	†Heads read in mercury.
81	"	29.37			69.0	3.626	
82	"	18.91			69.0	2.813	
83	"	11.55			69.0	2.127	
84	"	4.19			69.0	1.375	
133†	"	69.44	*B–C*	70.76	67.8	5.953	
134†	"	56.29	"	57.52	67.8	5.271	
135†	"	46.15	"	46.99	67.6	4.704	
136†	"	37.35	"	38.30	69.0	4.182	
137†	"	25.98	"	26.72	69.7	3.409	
138†	"	15.05	"	15.66	70.0	2.517	
139†	"	8.70	"	9.08	70.0	1.882	
372†	"	135.1	"	137.9	69.0	8.741	In 372 and 373 the discharge valve remained in the same position. They show the temperature effect on the discharge very clearly.
373†	"	135.3	"	138.2	56.5	8.497	
374†	"	206.4	"	210.8	57.5	10.872	
375†	"	173.9	"	177.7	58.0	9.854	
376†	"	103.3	"	105.3	55.3	7.261	
377†	"	66.8	"	68.1	55.8	5.651	
378†	"	32.1	"	32.9	56.3	3.726	
479†	"	198.3	"	201.4	37.9	10.077	Pipe *XIII* removed, 372 to 492.
480†	"	158.7	"	161.4	37.8	8.918	Air, 67°.
481†	"	124.2	"	126.2	37.0	7.685	
482†	"	92.3	"	93.9	37.0	6.513	
483†	"	57.9	"	58.8	37.1	4.975	
484†	"	19.1	"	19.4	37.4	2.714	Air, 66°.
485†	"	197.4	"	201.2	47.6	10.386	
486†	"	130.1	"	132.8	49.1	8.238	
487†	"	198.5	"	201.9	39.1	10.170	Air, 69°.
488†	"	198.4	"	201.8	41.1	10.206	
489†	"	199.0	"	202.6	43.9	10.266	
490†	"	283.5			48.5	12.751	Section *B–C* removed.
491†	"	286.5			39.9	12.534	
492†	"	233.1			43.0	11.348	

TABLE No. 4—(*Continued*).

BRASS PIPE *X*.

No. of Exp't.	Section.	Loss of head, in water. Feet per 100.	Section.	Loss of head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
71	*A-B*	48.38	.....	.....	70.3°	5.311	†Head read in mercury.
72	"	33.20	.....	.....	70.6	4.278	
73	"	21.19	.....	.....	70.7	3.301	
74	"	9.28	.....	.....	71.0	2.056	
75	"	2.59	.....	.....	71.0	1.107	
147†	"	76.72	*B-C*	76.77	68.3	6.914	
148†	"	59.75	"	59.78	68.0	5.972	
149†	"	40.00	"	40.16	68.0	4.772	
150†	"	29.57	"	29.75	68.0	3.895	
151†	"	17.06	"	16.55	68.1	2.939	
152†	"	11.17	"	11.54	68.3	2.333	
153†	"	3.93	"	3.95	68.7	1.335	
354†	"	51.05	"	51.09	68.1	5.483	
355†	"	50.99	"	50.95	56.6	5.340	
356†	"	78.43	"	78.31	54.0	6.796	
357†	"	40.44	"	40.56	56.2	4.681	
358†	"	15.93	"	16.00	55.2	2.749	
529†	"	181.9	"	181.4	64.8	11.321	
530†	"	148.2	"	147.8	55.2	9.823	
531†	"	114.6	"	114.3	51.0	8.411	
532†	"	180.0	"	178.7	74.1	11.387	
533†	"	139.1	"	137.8	73.4	9.818	
534†	"	173.9	"	173.9	42.5	10.462	
535†	"	145.0	"	145.2	42.0	9.385	
536†	"	110.2	"	110.6	44.0	8.057	
537†	"	74.6	"	75.0	45.2	6.450	
538†	"	171.7	"	171.3	38.0	10.276	
539†	"	127.6	"	127.8	37.1	8.636	
540†	"	92.6	"	93.0	37.2	7.182	
541†	"	56.4	"	56.5	37.2	5.388	
542†	"	32.8	"	32.7	37.1	3.970	
543†	"	149.4	"	149.0	36.7	9.445	

Brass Pipe *XIII.*

85	*A–B*	49.61			69.6*	4.115	†Heads read in mercury.
86	"	33.48			69.6	3.294	*Columns vibrating badly; vibrations greater in 293 than in 294; critical-velocity region.
87	"	17.21			69.6	2.250	
88	"	5.60			69.6	1.332	
140†	"	217.4	*B–C*	218.1	67.8	9.635	
141†	"	176.6	"	177.2	67.8	8.531	
142†	"	144.3	"	144.8	68.6	7.615	
143†	"	117.5	"	117.5	69.0	6.658	
144†	"	82.0	"	82.4	69.7	5.426	
145†	"	48.3	"	48.6	70.0	4.007	
146†	"	27.9	"	28.0	70.0	2.917	
293	"	7.11*	"	7.99*	60.6	1.765	
294	"	5.99*	"	6.56*	61.0	1.215	
295	"	4.47	"	4.47	61.5	1.270	
296	"	3.13	"	3.15	62.0	0.898	
297	"	1.85	"	1.87	62.1	0.540	
298	"	0.91	"	0.84	62.4	0.267	
319†	"	220.3	"	220.3	65.2	9.578	
320†	"	143.2	"	144.1	65.2	7.507	
321†	"	76.6	"	77.3	65.2	5.231	
330†	"	137.6	"	138.4	56.5	7.229	
331†	"	83.8	"	84.5	56.5	5.418	Air, 70°.
332†	"	192.5	"	193.5	56.5	8.775	
493†	"	406.7			63.2	13.651	
494†	"	300.3			60.8	11.43	
495†	"	408.3			46.2	13.20	Air, 67°. Removed *XI*, except entry length and piezometer *A*. End length of *XI*, with reducer, screwed into *A*. *XIII* *B–C* removed.
496†	"	353.7			48.6	12.22	
497†	"	282.4			47.4	10.71	
498†	"	213.2			46.8	9.097	
499†	"	209.2			38.2	8.777	
500†	"	302.5			36.3	10.780	
501†	"	402.5			37.4	12.78	
502†	"	252.9	*B–C*	253.6	50.0	10.07	*B–C* put on.
503†	"	199.7	"	201.2	50.0	8.836	
504†	"	124.3	"	125.1	47.0	6.664	
505†	"	81.7	"	82.0	46.0	5.227	
506†	"	26.4	"	27.2	39.0	2.744	
507†	"	64.0	"	63.9	38.8	4.443	Air, 65°.
508†	"	112.7	"	114.0	38.5	6.182	
509†	"	155.9	"	158.4	38.9	7.468	

TABLE No. 4—(*Continued*).

Brass Pipe *XII*.

No. of Exp't.	Section.	Loss of head, in water. Feet per 100.	Section.	Loss of head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.		Remarks.
76	*A–B*	55.18			72.0°	4.940		† Heads read in mercury.
77	"	39.17			72.3	4.002		
78	"	21.32			72.4	2.865		
79	"	9.53			72.5	1.809		
154†	"	205.9	*B–D*	187.8	68.3	10.477	10.165	
155†	"	160.4	"	146.3	68.0	9.050	8.780	
156†	"	107.7	"	98.64	68.0	7.232	7.016	
157†	"	80.08	"	73.15	68.0	5.903	5.727	
158†	"	46.63	"	42.56	68.1	4.454	4.321	
159†	"	31.16	"	28.41	68.3	3.536	3.430	
160†	"	11.74	"	10.47	68.7	2.023	1.963	
161			*B–C*	185.6	68.3	10.165		Experiments 161 to 167 were simultaneous with 154 to 160. The heads were read on a water gauge. The heads given for *B–D* (154 to 160) are really for *C–D*, the *B–C* heads (161 to 167) having been subtracted.
162			"	144.8	68.0	8.780		
163			"	97.4	68.0	7.016		
164			"	72.32	68.0	5.727		
165			"	42.27	68.1	4.321		
166			"	28.15	68.3	3.430		
167			"	10.52	68.7	1.963		
267†	*A–B*	135.4	*C–D*	124.2	68.8	8.270	8.024	
268†	"	73.22	"	67.4	69.2	5.817	5.644	
269†	"	133.0	"	122.2	65.0	8.137	7.895	
270†	"	104.4	"	95.94	65.0	7.088	6.877	
271†	"	64.35	"	59.34	65.0	5.362	5.208	
272†	"	48.35	"	44.59	65.0	4.556	4.422	
273†	"	28.92	"	26.61	65.0	3.390	3.290	
274†	"	207.2	"	190.2	65.3	10.510	10.197	
352†			"	125.2	68.1		8.061	
353†			"	125.0	56.6		7.850	Air, 68°.
359†			"	192.6	54.0		10.004	
360†			"	156.7	55.1		8.910	
361†			"	82.8	55.0		6.172	Air, 67°.
362†			"	34.6	56.0		3.738	
520†	*A–B*	202.7	"	186.1	45.4	9.907	9.612	
521†	"	145.8	"	133.4	45.4	8.170	7.927	
522†	"	81.2	"	74.2	45.7	5.843	5.669	
523†	"	288.7			54.9	12.425		*B–D* removed.
524†	"	249.5			53.0	11.375		Air, 71.5°.
525†	"	235.1			39.0	10.609		
526†	"	288.1			39.4	11.910		
527†	"	243.5			43.0	10.950		
528†	"	290.0			45.6	12.175		

Brass Pipe *XV.*

94	*A–B*	44.58			70.6°	2.892	†Heads read in mercury.
95	"	33.83			70.6	2.596	
96	"	19.26			70.6	2.362	
97	"	8.623			70.0	1.248	
98	"	2.001			70.0	0.300	
117†	"	180.2	*B–C*	194.2	69.5	6.761	
118†	"	155.4	"	167.6	70.0	5.946	
119†	"	129.6	"	139.5	70.0	5.384	
120†	"	99.9	"	107.6	70.2	4.680	
121†	"	67.84	"	72.47	70.4	3.730	
122†	"	13.60	"	20.72	70.9	1.953	
123†	"	275.8	"	299.3	70.6	8.304	
124†	"	218.4	"	235.6	71.0	7.249	
222†	"	510.5			74.8	11.868	*B–C* removed.
223†	"	411.4			75.5	10.519	
224†	"	319.1			76.0	9.102	
225†	"	207.7			75.7	7.109	
226†	"	114.2			75.1	5.075	
227†	"	45.32			73.9	3.009	
228†	"	29.05			73.1	2.594	
229†	"	18.16			73.0	1.952	
230†	"	9.33			73.1	1.362	
237†	"	278.4	*B–C*	303.3	72.9	8.398	
238†	"	179.2	"	194.5	70.9	6.537	
239†	"	122.7	"	133.7	73.6	5.277	
240†	"	273.4	"	298.4	70.7	8.217	Pipes Nos. 2 and 3 interchanged, placing Section *B–C* up stream.
241†	"	233.5	"	255.4	70.8	7.575	
242†	"	151.4	"	164.6	71.0	5.902	
253†	"	281.9	"	284.1	70.2	8.489	Pipes cleaned. *A–B* up stream.
254†	"	227.5	"	228.0	70.3	7.500	
255†	"	145.4	"	146.2	70.4	5.852	
256†	"	52.55	"	52.66	70.7	3.276	
257†	"	32.58	"	33.84	71.7	2.537	
258†	"	11.41	"	11.53	72.9	1.767	
368†	"	174.4	"	175.2	53.7	6.257	
369†	"	242.0	"	243.0	55.0	7.578	
370†	"	292.2	"	293.4	55.4	8.460	
371†	"	102.2	"	102.6	56.0	4.682	
413†	"	270.1	"	271.0	46.0	7.800	
414†	"	176.2	"	177.1	47.5	6.145	
415†	"	175.7	"	176.8	47.0	6.188	Air, 68°.
416†	"	109.8	"	110.4	49.6	4.781	
417†	"	52.3	"	53.0	45.7	3.183	Air, 69.5°.

TABLE No. 4—(*Continued*).

BRASS PIPE *XIV.*

No. of Exp't.	Section.	Loss of head, in water. Feet per 100.	Section.	Loss of head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
246†	*A-B*	304.0	*B-C*	308.8	70.2°	9.957	NOTE: Experiments 89 to 93, 125 to 132, 231 to 236, were rejected. The results on the two sections were not consistent, and when the pipe was taken down the ends were found to be jammed. (This pipe is very thin.) The jammed ends were cut off, and the pipes were refaced and rethreaded. Subsequent examinations showed no trouble.
247†	"	236.3	"	240.6	70.3	8.618	
248†	"	154.1	"	157.1	70.4	6.685	
249†	"	97.16	"	98.73	70.7	5.179	
250†	"	54.49	"	55.05	71.7	3.722	
251†	"	28.92	"	29.28	72.9	2.608	
252†	"	12.49	"	13.80	73.5	1.743	
363†	"	203.0	"	206.4	54.8	7.661	
364†	"	151.7	"	154.5	56.0	6.483	
365†	"	89.6	"	91.5	56.4	4.813	
366†	"	39.0	"	39.8	56.8	2.943	Air, 67.5°.
367†	"	310.5	"	317.1	54.1	9.760	
447†	"	202.0	"	204.7	47.2	7.427	Air, 76°.
448†	"	163.3	"	166.0	44.4	6.548	
449†	"	134.4	"	136.4	45.8	5.870	Air, 68.5°.
450†	"	96.4	"	97.9	42.4	4.859	
451†	"	61.5	"	62.2	42.8	3.780	
452†	"	35.7	"	36.6	42.2	2.825	Air, 66.5°.
453†	"	302.3	"	306.8	42.2	9.314	
454†	"	256.4	"	260.2	49.4	8.584	
455†	"	571.3	.....	.....	61.4	14.011	
456†	"	479.7	.....	.....	59.8	12.659	Air, 72°.
457†	"	373.1	.....	.....	59.8	10.936	
458†	"	266.8	.....	.....	60.8	9.047	
459†	"	265.3	.....	.....	42.5	8.625	Air, 71°.
460†	"	359.0	.....	.....	45.0	10.333	
461†	"	442.6	.....	.....	47.9	11.772	
462†	"	565.7	.....	.....	48.8	13.566	Air, 74°.
463†	"	555.7	.....	.....	37.8	13.036	Air, 68°.
464†	"	517.3	.....	.....	38.8	12.567	
465†	"	429.5	.....	.....	38.0	11.270	
466†	"	324.4	.....	.....	37.5	9.569	Air, 65.8°.
467†	"	216.5	.....	.....	37.6	7.553	
468†	"	93.3	.....	.....	37.6	4.792	Air, 65°.

† Heads read in mercury.

Brass Pipe *XVI.*

99	*A–B*	44.46	.....	.....	67.7°	2.676	† Heads read in mercury.
100	"	28.59	.....	.....	68.0	2.074	* Sudden change in gauge columns, probably due to the settling of the valve.
101	"	18.29	.....	.....	69.0	1.378	
102	"	7.804	.....	.....	69.5	0.602	
103†	"	178.6	*B–C*	178.4	70	5.135	
104†	"	136.7	"	136.6	70.2	4.424	
105†	"	105.4	"	105.2	70.9	3.840	
106†	"	76.4	"	76.6	71.0	3.280	
107†	"	48.29	"	48.6	71.2	2.721	
108†	"	24.15	"	23.0	71.9	1.790	
109†	"	60.61	"	62.36	69.5	2.917	
110†	"	56.53	"	58.49	70.0	2.866	
111†	"	37.36	"	38.42	70.0	2.559	
112†	"	31.94	"	31.93	70.2	2.309	
113†	"	11.72	"	11.71	70.4	0.911	
114†	"	5.86	"	5.97	70.9	0.451	
115†	"	271.4	"	276.6	70.6	6.542	
116†	"	229.9	"	234.7	71.0	5.993	
213†	"	512.8	.....	.....	74.5	9.448	*B–C* removed.
214†	"	423.0	.....	.....	75.0	8.467	
215†	"	323.9	.....	.....	75.1	7.277	
216†	"	231.1	.....	.....	75.2	6.001	
217†	"	124.6	.....	.....	75.3	4.255	
218†	"	47.25	.....	.....	75.1	2.020	
219†	"	32.24	.....	.....	75.1	2·282	
220†	"	19.89	.....	.....	75.1	1.614	
221†	"	8.97	.....	.....	75.2	0.730	
243†	"	276.6	*B–C*	280.5	70.7	6.606	Pipes Nos. 2 and 3 interchanged, placing section *B–C* up stream.
244†	"	227.0	"	230.2	70.8	5.816	
245†	"	136.2	"	137.9	71.0	4.437	
314†	"	178.0	"	180.7	64.3	5.107	Section *A–B* up stream.
315†	"	129.3	"	131.6	64.5	4.208	
316†	"	76.47	"	80.14	64.8	3.248	
317†	"	39.12	"	37.27	65.0	2.523	
318†	"	284.2	"	288.2	65.0	6.645	
322†	"	521.1	"	.....	64.0	9.337	*B–C* removed.
323†	"	446.1	.....	.....	64.0	8.552	
324†	"	324.5	.....	.....	64.0	7.143	
333†	"	496.8	.....	.....	55.0	8.940	
334†	"	388.2	.....	.....	55.8	7.809	

TABLE No. 4—(*Continued*).

BRASS PIPE *XV*—(*Continued*).

No. of Exp't.	Section.	Loss of Head, in water. Feet per 100.	Section.	Loss of Head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
418†	*A–B*	287.8			41.1	8.168	
419†	"	443.9			43.2	9.991	No. 419 seems erratic. No. 443 taken as a check.
420†	"	364.3*			46.4	9.254	*In No. 420 the head decreased by jumps, probably due to the valve setting slightly.
421†	"	440.7			38.2	10.237	
422†	"	374.3			38.0	9.327	
423†	"	314.1			37.7	8.450	
424†	"	255.7			37.6	7.522	Air, 69°.
425†	"	194.3			37.6	6.444	
426†	"	132.6			37.5	5.194	Air, 66°.
427†	"	71.4			37.6	3.752	
443†	"	440			43.7	10.344	
444†	"	461.4			45.8	10.695	
445†	"	408.7			46.2	9.965	
446†	"	325.9			46.1	8.759	Air, 71°.

TABLE No. 4—(*Continued*).

BRASS PIPE *XVI*—(*Continued*).

No. of Exp't.	Section.	Loss of Head, in water. Feet per 100.	Section.	Loss of Head, in water. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
335†	*A-B*	267.0*	.....	.....	56.5	6.327	Air, 62°.
336†	"	250.8	.....	.....	57.0	6.130	
337†	"	110.1	.....	.....	57.4	3.870	
428†	"	496.8	.....	.....	43.5	8.746	
429†	"	409.3	.....	.....	45.0	7.869	
430†	"	307.8	.....	.....	47.8	6.754	Air, 78°.
431†	"	307.4	.....	.....	39.4	6.659	
432†	"	389.9	.....	.....	38	7.562	
433†	"	436.2	.....	.....	38.3	8.047	
434†	"	481.0	.....	.....	38.6	8.516	
435†	"	267.9	*B-C*	271.1	39.0	6.140	Air, 65°.
436†	"	213.5	"	216.6	39.0	5.424	
437†	"	158.7	"	162.6	38.6	4.751	
438†	"	103.2	"	101.3	38.5	4.071	Air, 68°.
439†	"	66.7	"	64.9	38.5	3.142	
440†	"	33.7	"	33.0	38.5	1.635	
441†	"	19.1	"	18.8	38.4	0.958	
442†	"	7.2	"	7.2	38.3	0.387	

TABLE No. 5.—GALVANIZED PIPE *XVII.*

No. of Exp't.	Loss of head. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
592	70.89	35.5°	10.036	All heads measured in mercury.
593	39.90	35.8	7.493	
594	54.00	44.4	8.762	
595	39.59	42.5	7.452	Air, 64°.
596	25.85	41.2	6.008	
597	13.17	40.9	4.229	
598	5.88	41.2	2.795	
599	62.46	35.5	9.374	
600	55.35	35.6	8.476	
601	28.75	35.6	6.340	
602	16.51	36.4	4.766	
620	44.19	59.5	7.955	
621	26.21	51.2	6.092	
741	44.87	79	8.375	
742	43.89	43.6	7.813	
743	43.67	35.6	7.711	
744	74.36	35.4	10.155	Experimental pipe reversed (Piezometer *B* up stream) for these experiments.
745	73.74	35.2	10.118	
746	55.61	35.5	8.860	
747	26.82	35.2	6.041	
748	13.21	35.4	4.216	
749	7.37	35.6	3.111	

GALVANIZED PIPE *XVIII.*

631	51.61	44.6°	9.469	All heads measured in mercury.
632	41.11	42.9	8.321	
633	30.06	42.4	7.060	
634	20.60	42.0	5.708	
635	10.40	42.5	3.913	
636	4.99	42.8	2.618	
637	75.83	36.0	11.470	
638	64.87	36.0	10.554	
639	50.87	71.5	9.635	
640	32.41	64	7.548	
641	74.37	39.8	11.427	
642	74.89	35.8	11.405	
643	50.10	36.1	9.159	
644	35.74	36.2	7.757	
645	19.99	35.8	5.513	
646	8.47	36.5	3.423	
737	51.74	37	9.271	
738	31.00	37.2	7.047	Experimental pipe reversed (Piezometer *B* up stream) for these experiments.
739	15.52	38.2	4.811	
740	71.22	35.1	10.916	

TABLE No. 5—(*Continued*).—GALVANIZED PIPE *XIX*.

No. of Exp't.	Loss of head. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
622	96.62	55.0°	10.223	All heads measured in mercury.
623	61.20	49	7.909	
624	33.62	48	5.747	
625	16.72	48.2	3.894	
626	7.36	49.2	2.460	
627	143.31	77	12.783	
628	144.72	37.5	12.339	
629	80.22	37.5	9.049	
630	37.70	37.7	5.996	
755	98.04	78	9.994*	
756	97.56	50	10.079	
757	97.70	50.7	10.248	Experimental pipe reversed for these experiments.
758	78.17	52.0	9.028	
759	78.13	49.0	9.011	
760	39.71	48.5	6.267	
761	22.81	49.0	4.610	* Error in time? Velocity may be 10.316. See Experiment 768.
762	148.47	48.6	12.560	
763	149.69	37.1	12.578	
768	98.16	77.2	10.298	
769	77.00	80.2	9.196	
770	77.88	45.9	8.931	
771	97.65	45.8	10.062	

GALVANIZED PIPE *XX*.

603	166.6	61.5	10.885	All heads measured in mercury.
604	109.1	58.8	8.725	
605	165.6	37.2	10.653	
606	149.6	36.8	10.145	
607	116.5	35.9	8.877	
608	79.15	35.5	7.240	
609	46.20	35.6	5.499	
610	21.12	35.8	3.634	
611	10.34	39.0	2.487	
612	5.03	40.6	1.669	
613	113.3	42.6	8.781	
614	91.35	49.5	7.877	
615	112.6	47.0	8.775	
616	59.93	46.4	6.308	
617	112.2	77.2	8.896	
618	51.56	78.3	5.965	
619	164.5	79.2	10.884	
764	166.4	48.4	10.744	Experimental pipe reversed for these experiments.
765	108.2	45.2	8.607	
766	57.4	45.2	6.156	
767	25.9	52	4.077	

TABLE No. 5—(*Continued*).—GALVANIZED PIPE *XXI.*

No. of Exp't.	Loss of head. Feet per 100.	Temp. Fahr.	Velocity, in feet per second.	Remarks.
570	408.5	71.5°	11.411	All heads measured in mercury.
571	205.1	71.5	8.009	
572	341.5	68	10.394	
573	257.2	54	8.956	
574	181.3	52.2	7.489	
575	125.6	59.2	6.219	
576	84.71	57.8	5.085	
577	53.77	57.1	4.039	
578	31.68	59.8	3.067	
579	374.9	38.5	10.763	
580	311.8	38.6	9.830	
581	258.4	37.4	8.938	
582	195.3	37.4	7.739	
583	133.8	37.5	6.394	
584	84.05	38.0	5.025	
585	47.50	38.4	3.756	
586	5.97	42.6	1.305	
587	11.94	44.0	1.856	
588	25.69	47.2	2.744	
589	268.1	46.3	9.112	
590	142.6	47.8	6.621	
591	388.3	49.7	10.992	
750	316.5	45.1	9.795	Experimental pipe reversed (Piezometer *B* up stream) for these experiments.
751	315.4	36.4	9.651	
752	244.5	38.9	8.464	
753	161.7	36.4	6.834	
754	77.7	39.0	4.731	

REDUCTION TO STANDARD TEMPERATURES.

Brass Pipes.—After a few experiments had been made on each pipe, a preliminary logarithmic plotting of the results showed that for each of the fifteen brass pipes the loss of head varied very nearly as the 1.75 power of the velocity. Advantage was taken of this fact to bring the results to chosen standard temperatures. Plates VIII and IX were drawn, using heads in feet per 100 ft. as ordinates and the values of $V^{1.75}$ as abscissas. For any particular pipe on these plates, it will be noticed that there is a gradual increase in the ordinate as the temperature of the water drops, and that points of about the same temperature lie approximately on a straight line. Such lines were drawn for standard temperatures of 70, 55 and 40° Fahr. The difference in ordinates between two lines, divided by the ordinate, gives the ratio or percentage of increase or decrease in the loss of head for a change of temperature of 15 degrees. The percentage is always less for 40° than for 55°, as the ordinate itself is greater. In those cases in which the distances between the lines were different, thus resulting in two different percentages at 55°, the average of these

two percentages was taken. The three percentages, reduced to percentages per 10°, instead of 15°, were then plotted as ordinates, with temperatures as abscissas, and a smooth curve was drawn through the three points. The percentage correction per 10° for any observed temperature was read from these curves. The diagram used is shown in Fig. 3, in which it will be seen that for all pipes the correction factor is about 4 per cent. Another diagram was therefore constructed, from which the 4% corrections of the different heads for any temperature interval could be read, and the correction desired was then obtained by the slide-rule.

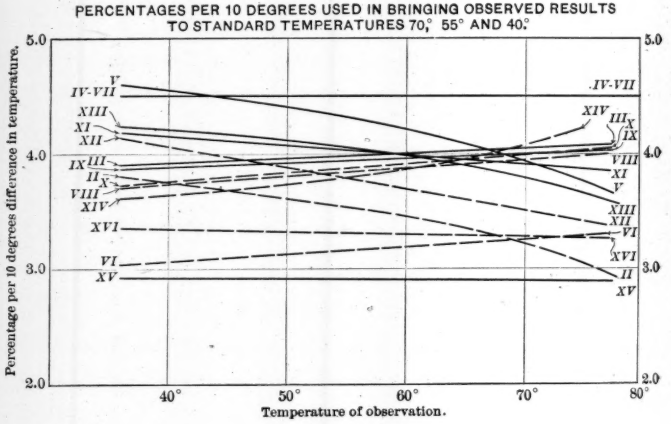

FIG. 3.

In Fig. 3 it will be noticed that for some pipes the percentage curves slope downward for increasing temperatures, and for other pipes there is an opposite tendency. While it is not to be maintained that these corrections are accurate, it is believed that they are correct within several tenths of 1 per cent. If this is true, a profile view of Fig. 3, at 70°, 55° or 40°, made by taking the percentages and plotting them with respect to the pipe diameters, gives indications of a sinuous relation between the temperature effects and the diameters. The evidence, however, is not conclusive enough to justify the statement that this is a fact.

Aside from errors in judgment in drawing the lines on Plates VIII and IX, there is at least one way in which small errors

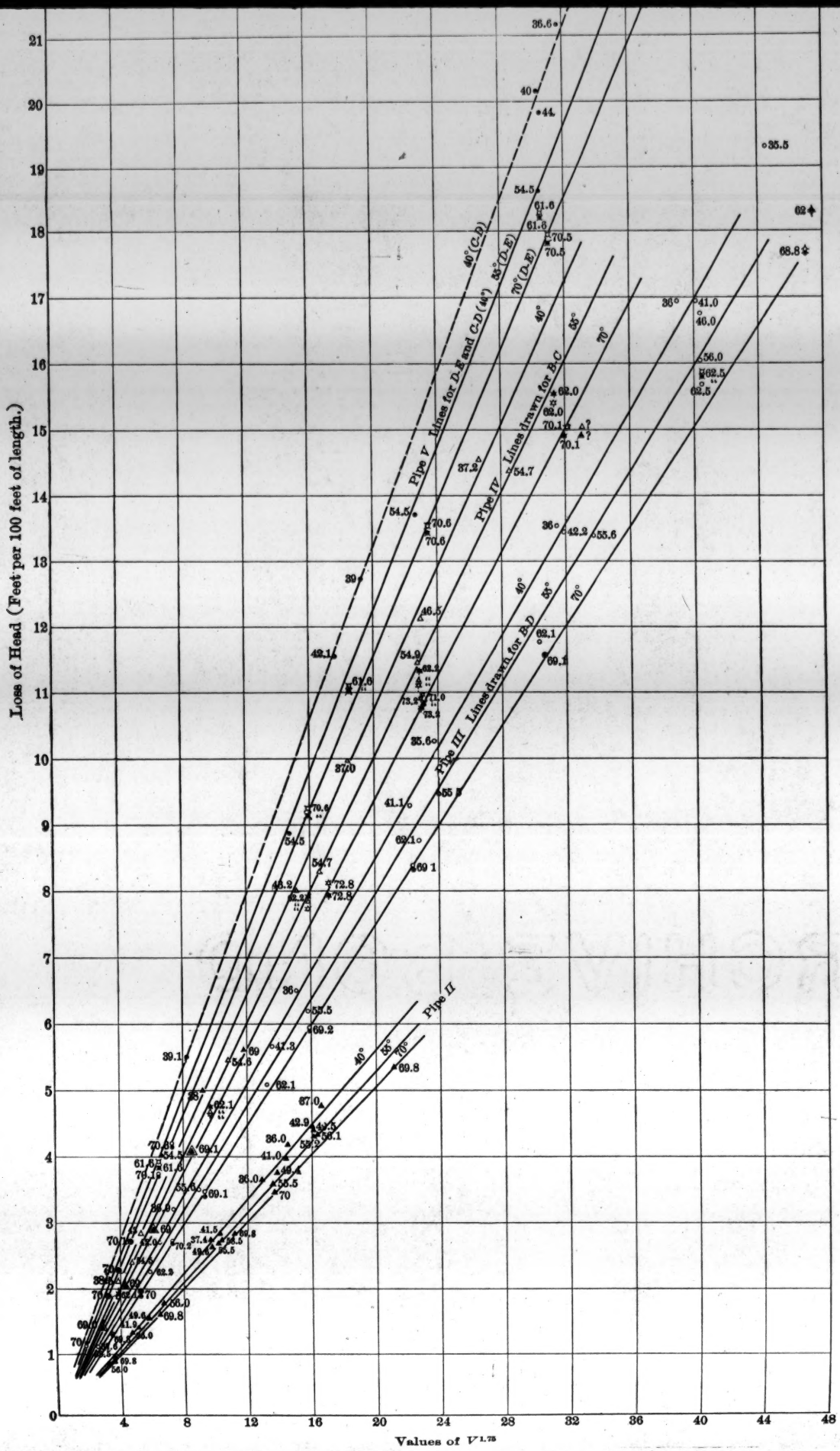
Loss of Head (Feet per 100 feet of length.)
Values of $V^{1.75}$
Pipe V Lines for D-E and C-D (40°)
40°(C-D)
55°(D-E)
70°(D-E)
Pipe IV Lines drawn for B-C
Pipe III Lines drawn for B-D
Pipe II
40°
55°
70°

PLATE VIII.
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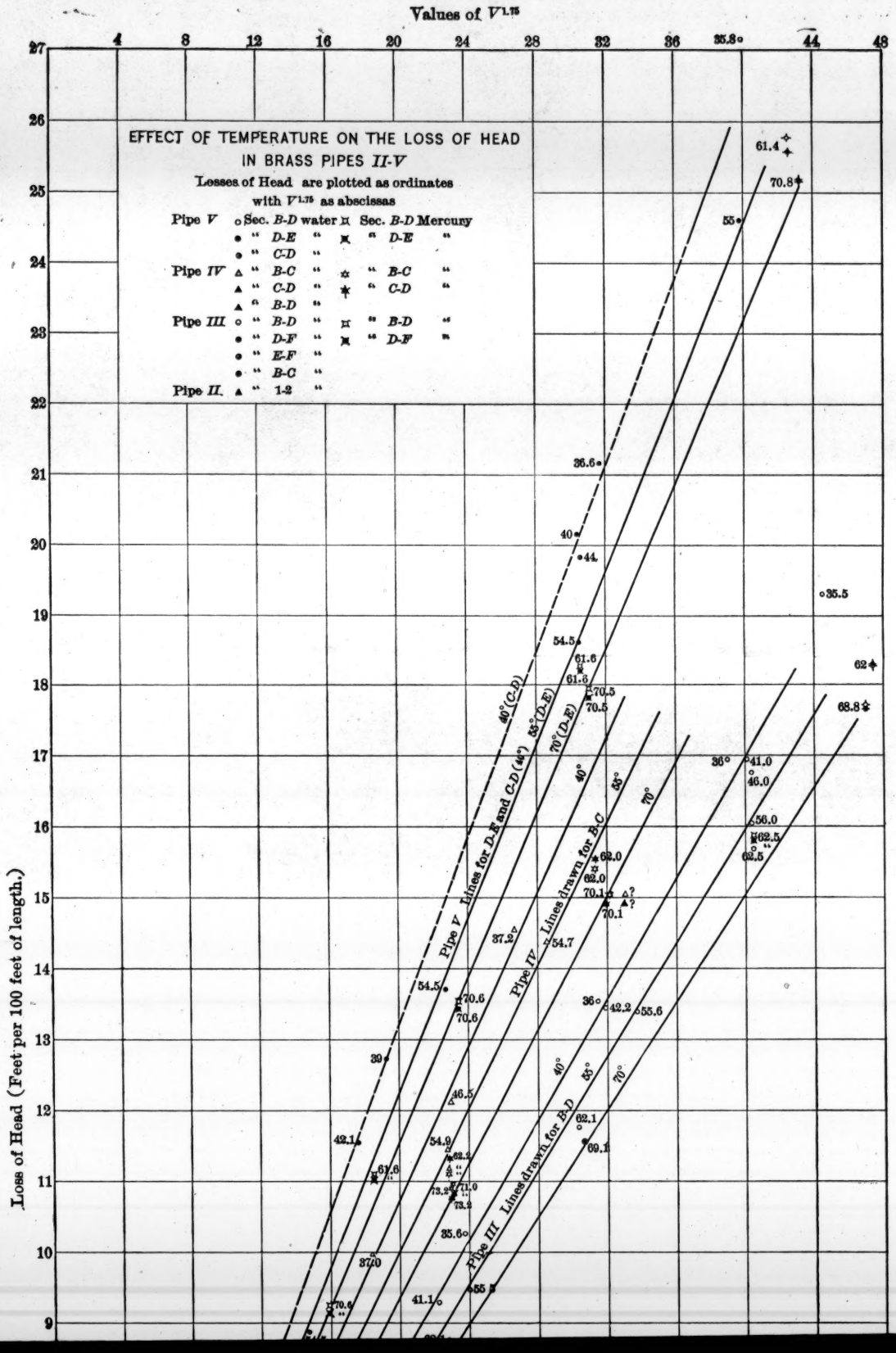
FLOW OF WATER IN PIPES.


EFFECT OF TEMPERATURE ON THE LOSS OF HEAD IN BRASS PIPES II-V
Losses of Head are plotted as ordinates with V^1.75 as abscissas
Pipe V ○ Sec. B-D water ⌑ Sec. B-D Mercury
● " D-E " ✖ " D-E "
● " C-D "
Pipe IV △ " B-C " ☆ " B-C "
▲ " C-D " ✱ " C-D "
▲ " B-D "
Pipe III ○ " B-D " ⌑ " B-D "
● " D-F " ✖ " D-F "
● " E-F "
● " B-C "
Pipe II. ▲ " 1-2 "
Values of V^1.75
Loss of Head (Feet per 100 feet of length.)
Pipe V Lines for D-E and C-D (44°) 55° (D-E) 70° (D-E) 40° (C-D)
Pipe IV Lines drawn for B-C
Pipe III Lines drawn for B-D
40°
55°
70°

might be introduced into the reduction factor. It will be seen later, in the tables for m and n, that for the different standard temperatures there are slight differences in the value of n for a certain pipe, and sometimes quite decided differences between these values of n and the preliminary value, 1.75. The differences in the values of n would result in changing the relative position of the lines, as they are all drawn upon the basis of $n = 1.75$. The fact that n is sometimes different from 1.75 results in giving a slight curvature to the line passing through the points at a certain temperature. As stated, however, the errors are not large, and, taking into consideration the fact that the correction intervals were in almost all cases less than 10°, it is certain that the limits of error mentioned have not been exceeded. In addition, the corrections were usually made from both sides of the standard temperatures, and any appreciable error would be evident at once in the plotting.

Another method for getting the temperature correction consists in plotting the values of $m = \frac{H}{V^{1.75}}$ as ordinates, with the temperatures as abscissas. When this is done, a straight line results in some cases and a curve in others. It usually gives the same results as the method already presented, but, taken altogether, it is not as reliable.

Galvanized Pipes.—For the galvanized pipes, a diagram was made, as for the brass pipes, using for the abscissas those approximate powers of the velocity which a preliminary logarithmic plotting showed for each pipe. These powers or values of n were quite variable, as the following values show.

	n
Pipe *XVII*	1.95
" *XVIII*	1.82
" *XIX*	1.82
" *XX*	1.90
" *XXI*	1.95

The diagram, however, did not give such consistent results as did the diagrams for brass pipes. The experiments were mostly in the neighborhood of 40 degrees. The results were usually somewhat erratic, and in many cases the estimate had to be based on the assumption of a uniform change from 70 to 37 degrees. Upon passing to the final plotting, however, these estimates, which varied from 1 to 2%

for 10° difference in temperature, showed themselves to be in error. More consistent reduction factors were obtained by using large-scale logarithmic paper for a preliminary plotting. Upon Pipes *XVII* and *XXI* very little evidence of temperature effect could be found, and the results were not corrected. For Pipe *XIX* 1.5% for 10° was found to apply; and for Pipe *XX* 1.25% was used. For Pipe *XVIII* the results were changed by 5% in the neighborhood of 40°, but, from all appearances, this would be too large in the vicinity of 70 degrees. Pipe *XVIII*, however, seems to be exceptional, in several respects.

Logarithmic Plotting of Results.—After being brought to the standard temperatures the results were plotted on regular, 10-in. base, logarithmic, cross-section paper,* plotting losses of head as ordinates and velocities as abscissas. The use of such paper is now so common that no explanation is considered necessary.

This plotting is shown in Plate X, which contains the results for both brass and galvanized pipes, plotted, however, for different origins. For the brass pipes, lines are drawn for the three standard temperatures, but for only one experimental section on each pipe, in order to avoid confusion.

Close inspection will show that in some instances the points do not lie exactly on the line. In such cases investigation was made to see if the reason existed in an error in the temperature reduction factor, but it was found that the differences were due to other causes. Sometimes the results obtained by mercury readings were slightly different from those taken in water. Altogether, however, the lines fit the points very well. Attention may be called here to the great uniformity in slope for the brass pipes.

Table No. 6 gives the values of m and n, as determined from the logarithmic plotting. In cases, however, where the points do not lie exactly on a straight line, or where, as in Pipe *XVI* at 40°, the points which determine the line are few in number, and for high velocities, there is usually a little uncertainty in drawing the line. In this way, small variations in m may be brought about, but when it is considered that there is a corresponding change in n, it will be seen that the two values taken together will represent the conditions quite accurately.

* The writers were supplied with this paper by John R. Freeman, M. Am. Soc. C. E., whose generosity and kindly interest they desire to acknowledge.

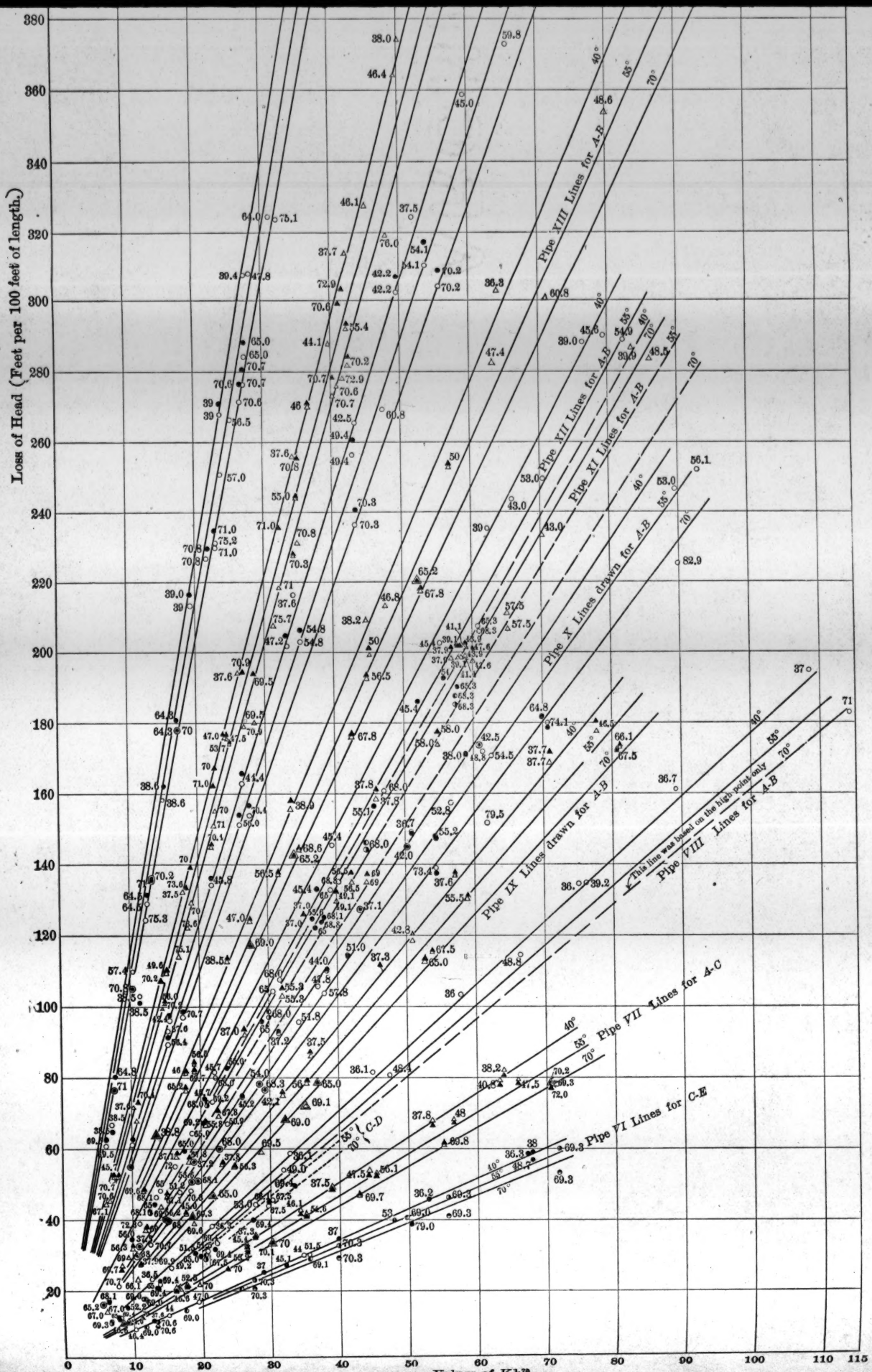

Loss of Head (Feet per 100 feet of length.)
Values of V^1.75
Pipe XIII Lines for A-B
Pipe XII Lines for A-B
Pipe XI Lines for A-B
Pipe X Lines drawn for A-B
Pipe IX Lines drawn for A-B
Pipe VIII Lines for A-B
This line was based on the high points only
Pipe VII Lines for A-C
Pipe VI Lines for C-E
C-D
40°
55°
70°

PLATE IX.
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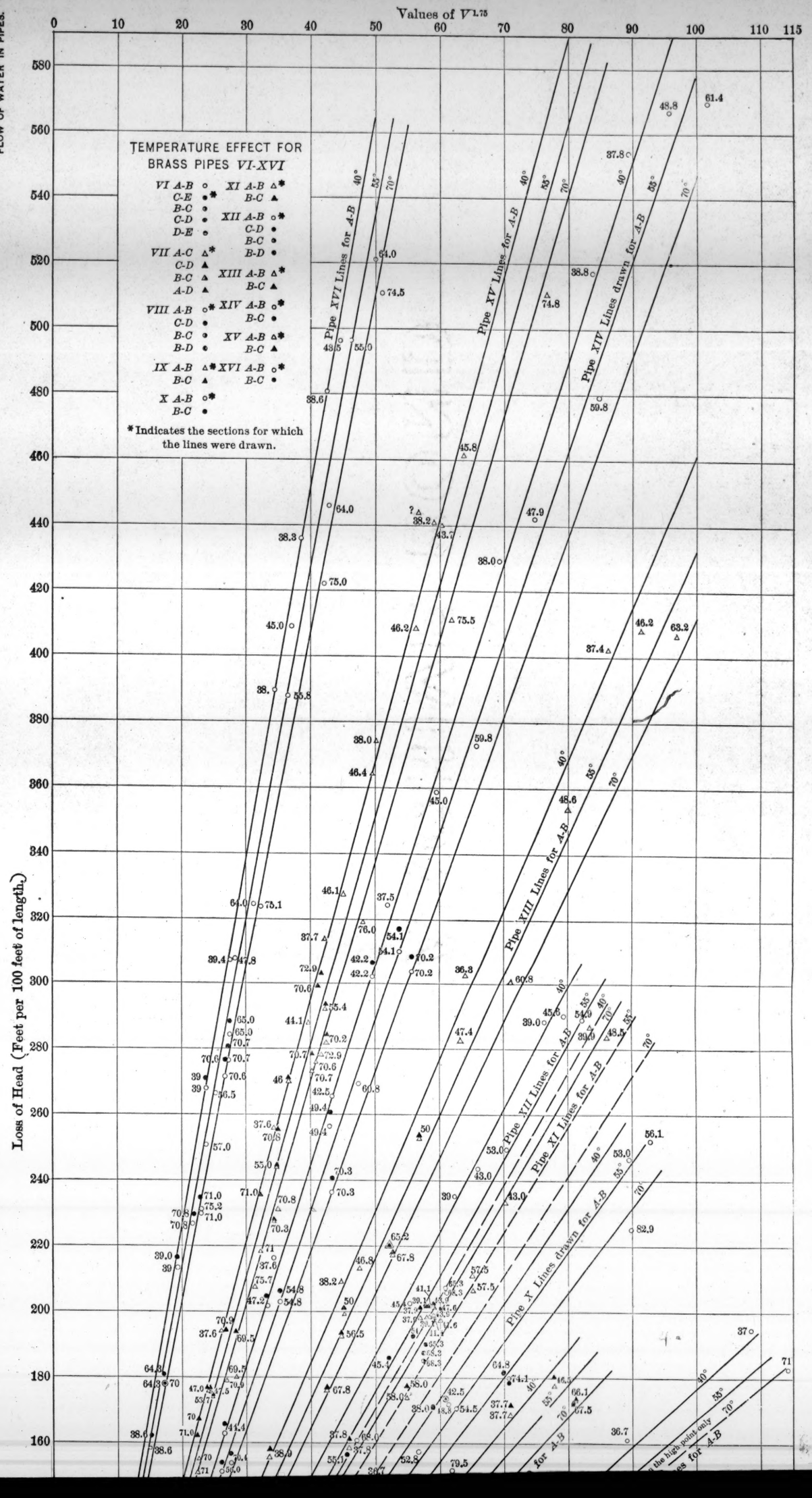


In Table No. 6 there will also be found the values of m, as taken from Plates VIII and IX on which the points were plotted with heads as ordinates and the values of $V^{1.75}$ as abscissas. The slopes of the lines drawn must give the values of m corresponding to the power, 1.75, of the velocity. Upon comparing these values of m with those obtained from the logarithmic plotting, it is seen that they check quite well. These values of m are greater or less than the values determined logarithmically, accordingly as 1.75 is greater or less than the true n. Taken altogether, with these values of m, and $n = 1.75$, we can represent closely the whole series of experiments.

The method of least squares could also be applied in getting the values of m and n. This was tried in the case of Pipe *II*, and gave a close check upon the values taken from the logarithmic sheet. The method of least squares, however, is not as convenient, and does not expose the errors which may occur.

The differences in results for two sections of the same pipe do not seem to be accounted for through differences in diameter, except in Pipes *VI*, *VIII* and *XII*, where differences were known to exist and were allowed for in the calculations. In the other pipes small differences in resistance are noted, but must be accounted for otherwise. As first used, the two experimental sections of Pipe *XV* showed very discordant results, as the table and the logarithmic plotting show. After being cleaned by drawing waste cotton through, the results were more consistent. During the cleaning a small scale of brass was drawn out, and it seems very probable that the small differences noted in other pipes may be due to similar causes.

Table No. 7 gives the values of m and n for the galvanized pipes. As shown by the logarithmic plotting, Pipe *XVIII* seems to be an exceptional pipe. Even though of smaller diameter than Pipe *XVII* the losses of head, for velocities above 2.4 ft. per second, are lower than in Pipe *XVII* for the same velocity. It will be remembered, also, that Pipe *XVIII* is the one which showed high temperature effects in the neighborhood of 40°, and that it is the only one of the galvanized pipes which had been in use before the experiments. A slight, silt-like deposit which had occurred on the inner walls was entirely insufficient to relieve the roughness. Although the galvanized pipes do not seem to give such consistent results as the brass pipes, the experiments were made with the same care.

		40	3.90	1.731	3.74		40	3.72	1.738	
	B–C	70	3.25	1.739	.....					
	B–D	70	3.38	1.736	.....					
XIII	*A–B*	70	4.25	1.733	4.11	*B–C*	70	4.25	1.733	
		55	4.45	1.730	4.32		55	4.45	1.730	
		40	4.70	1.743	4.61		40	4.70	1.743	
XIV	*A–B*	70	5.52	1.745	5.46	*B–C*	70	5.52	1.755	
		55	5.80	1.747	5.78		55	5.95	1.740	
		40	6.28	1.742	6.13		40	6.45	1.731	
XV	*A–B*	70	6.36	1.774	6.76	*A–B*	70	5.66	1.764	*A–B* and *B–C* in last columns before cleaning.
		55	6.71	1.768	7.05	*B–C*	70	7.12	1.765	
		40	7.07	1.771	7.36					
	B–C	70	6.36	1.774	.....					
		55	6.71	1.768	.....					
		40	7.07	1.771	.....					
XVI	*A–B*	70	9.78	1.771	10.21	*B–C*	70	9.23	1.785	
		55	10.13	1.780	10.69		55	9.88	1.794	
		40	10.28	1.795	11.25					

TABLE No. 6—Brass Pipes *II* to *XVI*, Inclusive.

Values of m and n (m for Head in Feet per 100.)

Pipe.	Section.	Temp. Fahr.	m.	n.	m from $V^{1.75}$ diagrams.	Section.	Temp. Fahr.	m.	n.	
II		70°	0.252	1.749	0.253					
		55	0.267	1.748	0.268					
		40	0.282	1.750	0.282					
III	*B–D*	70	0.372	1.754	0.377	*D–F*	70°	0.375	1.750	
		55	0.386	1.767	0.400	*E–F*	70	0.357	1.776	Few points for *E–F*.
		40	0.420	1.755	0.425					
IV	*B–C*	70	0.485	1.744	0.472	*C–D*	70	0.497	1.730	
		55	0.507	1.753	0.503		55	0.525	1.741	
		40	0.545	1.744	0.550					
V	*D–E*	70	0.584	1.747	0.580	*B–D*	70	0.584	1.747	
		55	0.600	1.763	0.613	*C–D*	40	0.660	1.755	
		40	0.660	1.755	0.660					
VI	*C–E*	70	0.770	1.737	0.788	*D–E*	70	0.729	1.755	
		40	0.880	1.738	0.864	*C–D*	70	0.850	1.722	
	A–B	55	0.850	1.738	0.822	*B–C*	70	0.815	1.758	
		40	0.895	1.743	.....					
VII	*A–C*	55	1.165	1.753	1.172					
		40	1.340	1.716	1.100	*B–C*				
	C–D	55	1.170	1.748	.....	*A–C*	70	1.11	1.744	All at 70° were made before pipe was set up the second time.
		40	1.340	1.709	.....	*C–D*				
VIII	*A–B*	70	1.53	1.732	1.615	*C–D*	70	1.51	1.737	$V^{1.75}$ diagram for *C–D VII* gives $m = 1.47$ and 1.55.
		55	1.630	1.756	1.675		55	1.64	1.730	
		40	1.74	1.757	1.775					
IX	*A–B*	70	2.04	1.763	2.10	*B–C*	70	2.05	1.760	
		55	2.20	1.751	2.32		55	2.22	1.749	
		40	2.42	1.740	2.36		40	2.40	1.750	
X	*A–B*	70	2.60	1.746	2.58	*B–C*	70	2.60	1.746	
		55	2.72	1.753	2.73		55	2.75	1.745	
		40	2.94	1.741	2.90		40	2.94	1.741	
XI	*A–B*	70	3.08	1.746	3.02	*B–C*	70	3.12	1.744	
		55	3.22	1.747	3.21		55	3.26	1.749	
		40	3.45	1.743	3.41		40	3.51	1.747	
XII	*A–B*	70	3.39	1.744	3.34	*C–D*	70	3.27	1.739	
		55	3.61	1.739	3.52		55	3.48	1.738	

TABLE No. 7.

GALVANIZED PIPES *XVII* TO *XXI*, INCLUSIVE. VALUES OF m AND n (m FOR HEAD IN FEET PER 100).

Pipe.	Temp. Fahr.	m.	n.
XVII		0.820	1.931
XVIII	40°	0.905	1.802
XIX	40	1.320	1.863
XX	70	1.74	1.906
	40	1.81	1.910
XXI		3.52	1.961

CRITICAL VELOCITY.

One of the most interesting features of Plate X, Fig. 1, is the evidence regarding critical velocity. As might be expected, this phenomenon is most prominent on Pipe *XVI*, where the peculiar effects are evident at relatively high velocities. For low velocities, up to 2.5 ft. per second, the slope is 1, showing that the loss of head is directly proportional to the velocity. Above a velocity of 3.5 ft. the slope is about 1.78. Between these velocities there is a region where the slope gradually changes and reaches a value considerably greater than 2. The cause, as Osborne Reynolds, F. R. S., has shown* is a change in the conditions of flow.

The effect of temperature on the point at which the change takes place is well shown by Pipe *XVI*, and also in Fig. 4, which has been prepared from the observations of Hagen.† For these experiments $0.5\frac{V^2}{2g}$ was allowed for head lost at entrance (square-edged). This may not be quite correct. The pipes were short, being 1.55, 3.57 and 3.42 ft. long, for the small, medium and larger sized pipes, respectively. Any other allowance for entry would not change the relative position of the lines to any great extent. The figure has been introduced to show the effect of temperature in the critical-velocity region, particularly for greater temperatures than the writers could obtain, and also to verify the indications of the present experiments. The principal point to be brought out is that the change takes place at higher velocities for lower temperatures.‡ The change to the power

* *Proceedings*, Royal Society, London. Vol. 35, 1883.

† "Abhandlungen der Berliner Akademie," 1854.

‡ Some recent experiments substantiating this very point came to the writers' notice after this paper had been written. They are by Messrs. E. G. Coker and S. B. Clement, and a record of them appeared in the *Transactions* of the Royal Society, Series A, Vol. 201, pp. 45-61. A single brass pipe, 0.3779 in. in diameter and 6 ft. long, was used. The experiments, also, check closely the temperature factor obtained by the writers for ordinary, or "eddy motion," flow. The range of temperatures was from 4 to 50° Cent., or 39 to 122° Fahr. The interesting point is made that the intersections of the lines for ordinary and for "stream line" flow vary as the viscosity of the water.

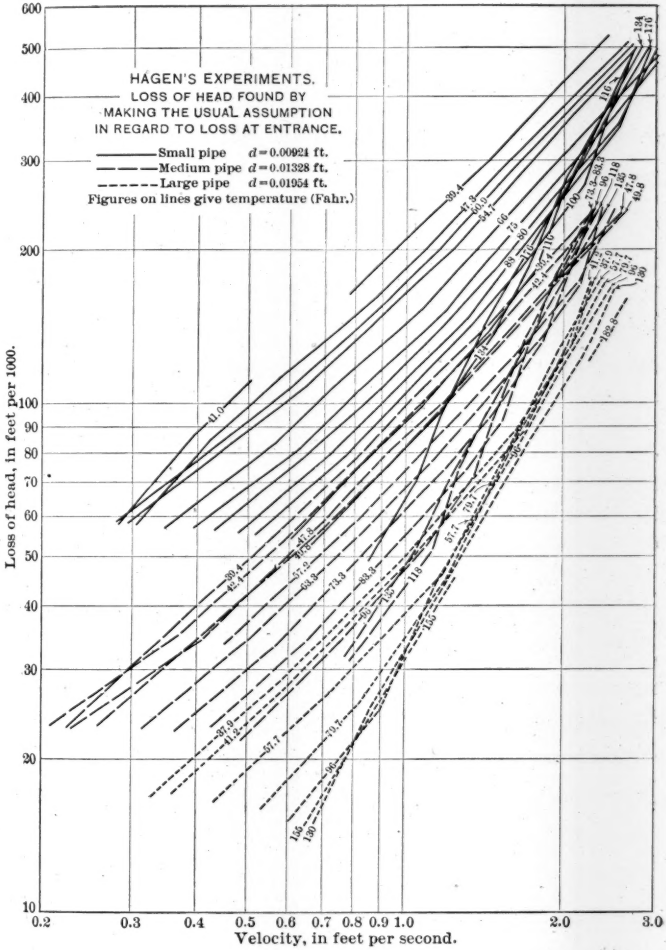

HAGEN'S EXPERIMENTS.
LOSS OF HEAD FOUND BY
MAKING THE USUAL ASSUMPTION
IN REGARD TO LOSS AT ENTRANCE.
Small pipe d = 0.00924 ft.
Medium pipe d = 0.01328 ft.
Large pipe d = 0.01954 ft.
Figures on lines give temperature (Fahr.)
Loss of head, in feet per 1000.
Velocity, in feet per second.
600
500
400
300
200
100
90
80
70
60
50
40
30
20
10
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1.0
2.0
3.0

FIG. 4.

1.75, takes place on Pipe *XVI* at a velocity of 3.5 ft. at 70° and at about 5 ft. for 40°. Particular attention is also called to the crossing of the lines, and to the fact that where the loss is proportional to the first power of the velocity the difference in head for any certain temperature interval is many times greater than where the loss is proportional to the 1.75 power of the velocity. For Pipe *XVI* a line is also obtained for 75 degrees. In some pipes, Pipe *IX* for example, where two sections show very slight differences under regular conditions of flow, the differences are magnified below the critical velocity.

The interesting question, however, in connection with the critical velocity, is whether it is an element to be considered for the velocities and pipe diameters used in practice. On the basis of the experiments on brass pipe, and assuming that there is a continuity of law where extended to diameters greater than those experimented upon, the question may be given an approximate answer. The question involves the determination of the relation between the pipe diameter and the velocity at which the power of the velocity ceases to be 1.75.

On Plate X the points for Pipes *XVI*, *XV*, *XIII*, *IX* and *VII*, may be determined fairly well for the experiments at approximately 70 degrees. In addition to these points, the writers have also the evidence of experiments at very low velocities on Pipe *II*. These were made early in 1901, in connection with another study in hydraulics. The heads were measured first with water and later with oil gauges. The region of critical velocity was explored thoroughly. No evidence of the phenomenon was found in the first 68 ft. of pipe down stream from the 5-in. pipe. For this section the ordinary-flow equation seemed to hold, down to velocities as low as 0.25 ft. per second. But, in the succeeding length, corresponding to Section *A–B* in the present experiments, the gauges showed a decidedly unstable condition of flow from 0.55 ft. per second down to about 0.35 ft. per second. The temperature of the water for these experiments was about 40 degrees.

The critical-velocity points seem to lie approximately on a straight line. Such a line was drawn, and the intersections with the 70° lines of slope, 1.75, were marked. The velocities thus determined were then plotted logarithmically, with respect to the diameters, and from this plotting the law for impending critical velocity at 70° appears to be $V = \frac{0.088}{d^{0.784}}$. This expression, being only roughly approxi-

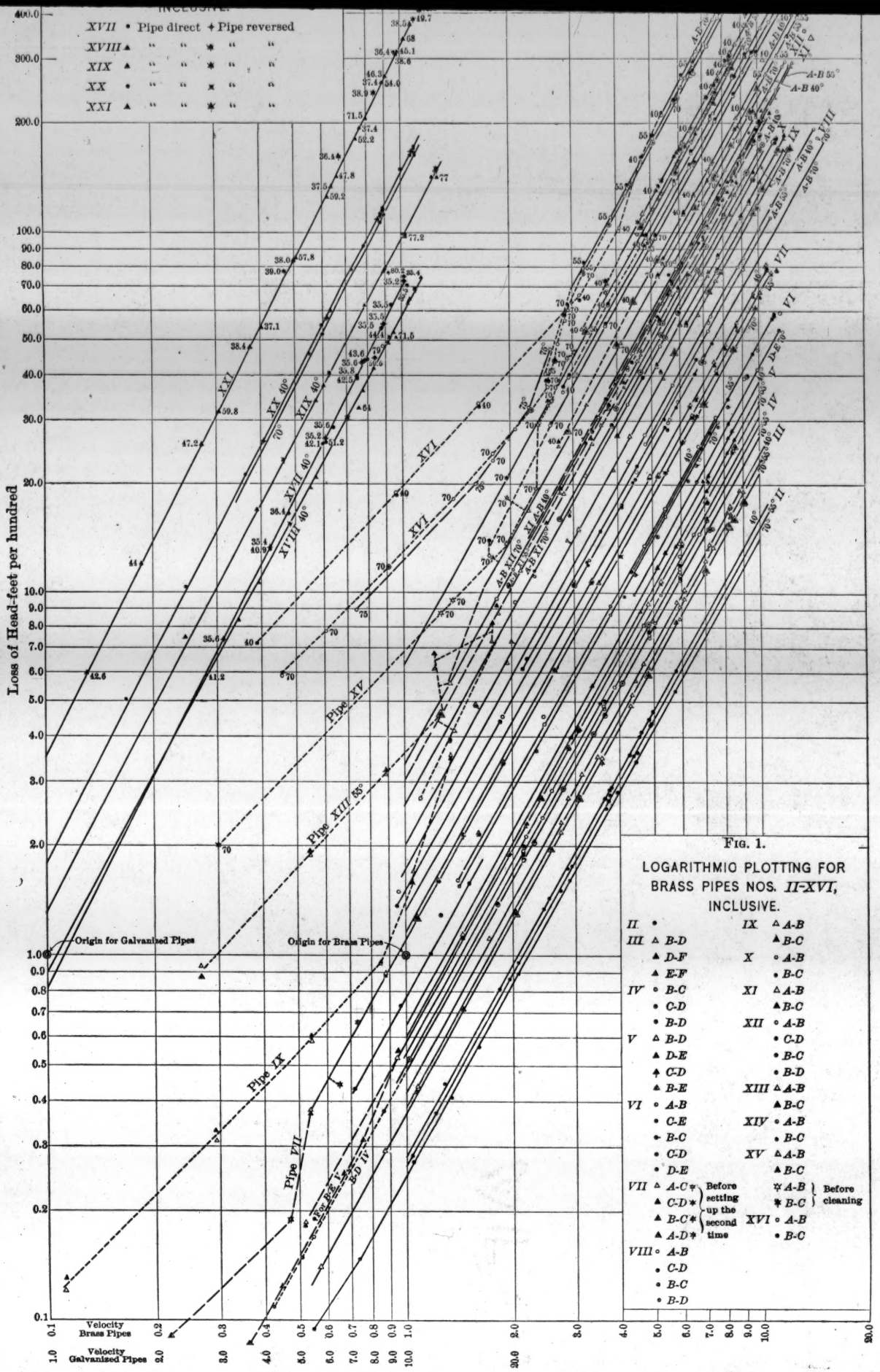
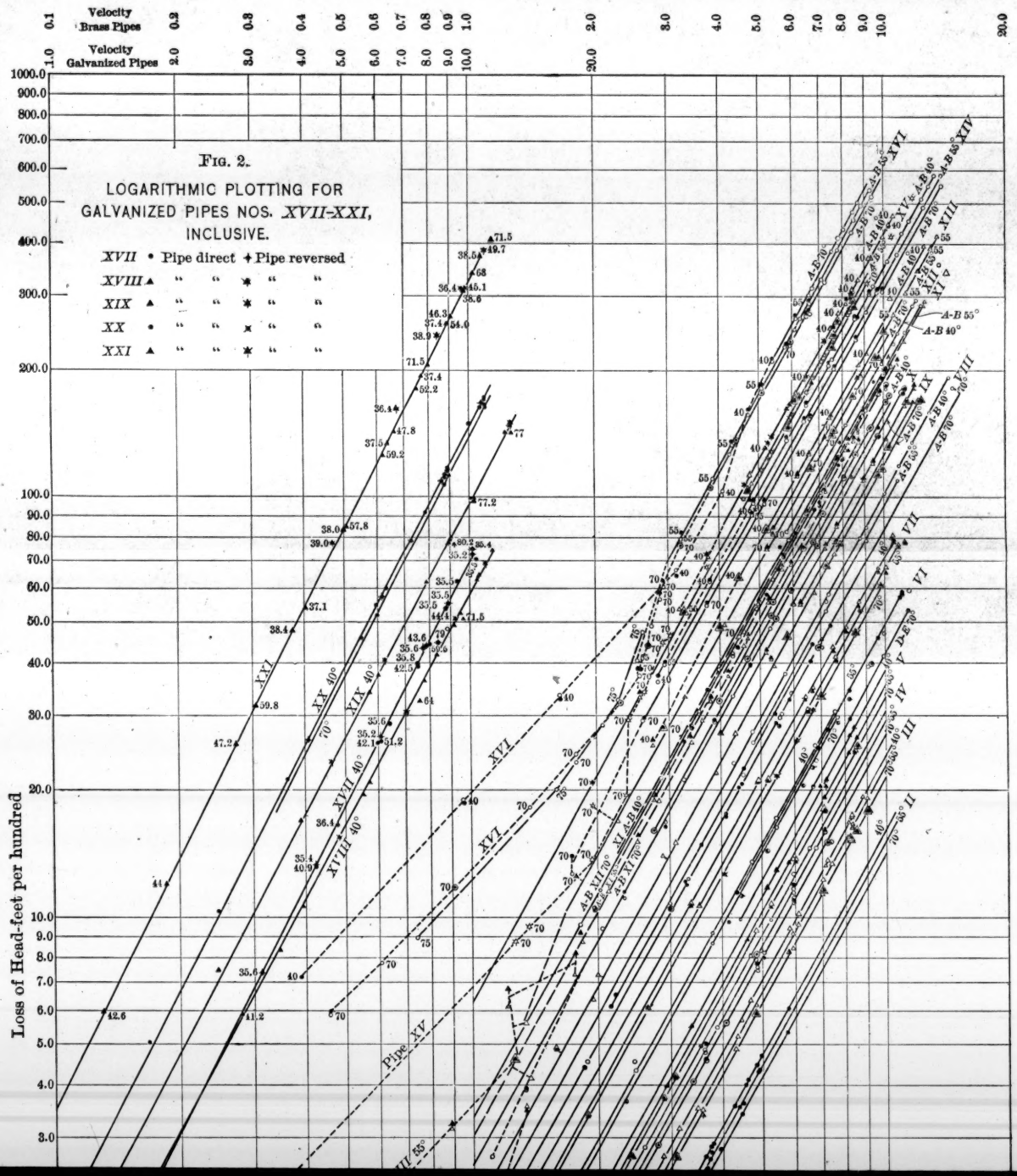


FIG. 1.

LOGARITHMIC PLOTTING FOR BRASS PIPES NOS. II-XVI, INCLUSIVE.

PLATE X.
TRANS. AM. SOC. CIV. ENGRS.
VOL. LI, No. 964.
SAPH AND SCHODER ON
FLOW OF WATER IN PIPES.


Velocity Brass Pipes
Velocity Galvanized Pipes
Loss of Head-feet per hundred
FIG. 2.
LOGARITHMIC PLOTTING FOR GALVANIZED PIPES NOS. XVII-XXI, INCLUSIVE.
XVII • Pipe direct + Pipe reversed
XVIII ▲ " " * " "
XIX ▲ " " * " "
XX • " " * " "
XXI ▲ " " * " "
Pipe XV

mate, may be replaced by $V = \frac{0.1}{d^{0.75}}$ (d, in feet). For 40° there are only two points, namely, Pipes *XVI* and *II*, but these indicate the law expressed by $V = \frac{0.15}{d^{0.75}}$.

The older series of experiments do not include many with velocities below 1 ft. per second, and the critical velocity, therefore, is not easily determined. They seem to indicate, however, that the velocities at which ordinary conditions are established are somewhat lower than those given by the foregoing formula. However, it is seen from the formula that the change in condition takes place at very low velocities for ordinary sizes of pipe. Moreover, as appears from Pipe *II*, it seems that, for large sizes, a long length of straight pipe is necessary to allow the below-critical-velocity flow to adjust itself.

The formula given determines only the limiting point for the use of the power, 1.75. The lower points, where the loss ceases to be directly proportional to the velocity, are approximately on the same line that was drawn in the other case, but the intersections must now be taken with the lines of slope 1. The law seems to be about $V = \frac{0.045}{d^{0.85}}$. This law, however, is not of as much interest as the one applying to the upper points.

It seems that the question of critical velocity, while of considerable scientific interest, is of no practical importance to the engineer; but space has been given to its consideration on account of the fact that many scientists have been led astray in attempting to apply experimental data from very small pipes to pipes used by the engineer, simply because the change in the manner of flow was not understood.

Experiments on Small Rubber Hose.

It had been planned to perform a considerable variety of experiments on small hose, especially as regards the effect of curvature on the loss of head. Unfortunately, there was not enough time to follow out by a more extended experimental study the interesting and surprising results obtained.

The hose were fed from the bottom of the large drum, 1.135 ft. internal diameter and 2.167 ft. long, at the up-stream end of the 5-in. brass pipe. A bushing, a 1-in. short vertical nipple, a 1-in. reducer-elbow, a $\frac{3}{4}$-in. short nipple, a $\frac{3}{4}$-in. disc valve, and bushings into which the connecting nipples of the hose screwed, were the fittings, in their

respective order. The valve was wide open during the experiments, the pressure being regulated by a 2-in. valve on the pipe leading into the drum. An air-chamber was attached to the top of the drum, and from this a connecting hose led to a two-column mercury gauge, one arm of which was open to the air. In all the experiments the hose were allowed to drop to the floor (about 6 ft. below the bottom of the drum) and laid straight (curve experiments excepted, of course) along it, except for an easy return curve of about 3 ft. diameter and a slow rise to the sharp downward curve at the discharge end. Thus the entry and discharge conditions were alike in all experiments. The level of the discharge end, with respect to the gauge scale, was determined. An extreme variation of 0.1 ft. might have occurred.

In Experiments Nos. 655 to 658 the hose was laid on a board and held in position in a horizontal plane by means of nails. The loops occurred at the ends of the board where the hose returned on itself. Except at the ends, the curves were semicircles alternating in direction of curvature, the radius being the same as when the hose was wound on the cylindrical drum.

The hose used were of three-ply, cotton-insertion, rubber tubing with smooth interior surfaces. The inside rubber lining was very heavy.

It has been the established idea that reversal of curvature introduces about twice as much loss of head as the same amount of continuous curvature, the length of pipe being the same in the two cases. The experiments on Hose *A* contradict this so flatly that it can only be concluded that there is something radically wrong with the hypothesis underlying the accepted notion. The small size of the hose might be regarded as a bar to the serious consideration of the results, but the experiments of John R. Freeman, M. Am. Soc. C. E., on 2.49-in. hose* are somewhat confirmatory. Mr. Freeman found that a full circle of 2 ft. radius (9.65 diameters) gave practically the same excess loss over straight hose as that given by four quadrants of the same radius, but introducing two reversals of curvature. In figures, the full circle gave $5\frac{1}{2}\%$ less excess loss.

In the curvature experiments on Hose *A*, the data of which are given in Table No. 8, the radius of curvature was $2\frac{15}{16}$ ins., or nearly eight diameters. The amount of curvature was purposely made large so that the effects might show up in no uncertain manner.

* *Transactions*, Am. Soc. C. E., Vol. XXI.

TABLE No. 8.—Experiments on Hose.

Hose A. $l = 47.93$ ft. $d = 0.370$ ins.

No. of experiment.	Temp.	Total acting H, feet of water.	Loss of head, in feet per 100.		Velocity.	Remarks.
647	47.5°	49.01	98.7		8.431	$0.5\frac{V^2}{2g}$ allowed for entry; 647 to 650 hose laid with two easy curves in addition to one at beginning and one at end; 659 same; 651 to 654 hose wound in a spiral of 22 complete turns around a drum of 5¼ ins. diameter; 655 to 658, 39 curves, including 3 loops; 38 reversals of curvature: curves of same radius as in 651 to 654. In curve experiments there were curves also at beginning and end as in 647 to 650.
648	.....	38.14	77.0	If laid as in 647 to 650.	7.343	
649	.....	27.03	54.6		6.066	
650	49	14.18	28.7		4.228	
651	.....	49.10	99.9		7.248	
652	49	38.14	77.6	75.5	6.371	
653	49.5	23.37	47.5	60.0	4.886	
654	51	11.23	22.9	37.3	3.241	
655	51	49.10	99.4	17.9	7.733	
656	51.2	38.14	77.3	84.6	6.760	
657	.....	25.16	51.0	66.6	5.423	
658	53.0	12.34	24.9	45.0	3.692	
659	51	49.05	98.9	22.6	8.418	

Hose A_1. $d = 0.380$ in.

684	49	49.09	94.8	.....	7.640	$0.5\frac{V^2}{2g}$ allowed for entry.
685	49.5	34.59	66.7	.....	6.307	
686	50.5	16.73	32.4	.....	4.273	$l = 50.28$ ft.
678	60	49.22	47.9	.....	5.488	Used $A + A_1$ ($l = 98.58$ ft.) and subtracted A, using a diagram based on Experiments 647 to 650.
679	61	36.63	35.4	.....	4.674	
680	62.2	22.08	21.6	.....	3.504	
681	63.5	8.86	8.7	.....	2.103	
682	50	49.03	49.1	.....	5.393	
683	42.4	74.05	73.8	.....	6.787	

For A_1.

Hose B_1. $l = 11.87$ ft. $d = 0.504$ in.

687	45	48.10	340.5	.....	18.076	$0.5\frac{V^2}{2g}$ allowed for entry.
688	44.5	36.98	262.5	.....	15.781	
689	44	24.79	176.3	.....	12.854	
690	44	14.71	104.9	.....	9.829	
691	44	7.85	56.3	.....	7.078	
692	36.5	72.26	511.1	.....	22.240	

Hose B. $d = 0.519$ in.

693	43.5	48.80	77.1	.....	7.731	Used $B_1 + B$ ($l = 61.82$ ft.) and subtracted B_1, using a diagram based on Experiments 687 to 692.
694	43	33.68	53.2	.....	6.370	
695	43.5	18.77	29.9	.....	4.649	
696	44	9.71	15.7	.....	3.243	
697	39	74.39	116.7	.....	9.728	

For B.

Hose C. $l = 14.37$ ft. $d = 0.254$ in.

698	69	49.56	325.9	.....	10.750	$0.5\frac{V^2}{2g}$ allowed for entry.
699	60.5	36.63	240.8	.....	9.255	
700	58.5	24.73	162.8	.....	7.520	
701	58	14.48	95.6	.....	5.627	
702	58	8.17	54.1	.....	4.082	
703	60	4.53	30.1	.....	2.973	
704	63	49.31	324.4	.....	10.757	
705	50.5	49.14	322.7	.....	10.815	
706	47	48.99	321.9	.....	10.750	
707	44	75.00	490.7	.....	13.764	
708	43	49.81	327.2	.....	11.088	

The most surprising result was, that, although each series showed a most decided effect on the loss of head and the discharge, the value of n, in the formula $H = m\ V^n$, remained practically constant, the large changes being in the value of m. (There is a small increase for the case of reversed curvature.) Should this result be confirmed by further experiments along this line, it will have an important bearing on the subject of the nature of the resistances which cause loss of head in a pipe. For it would appear that the value of the exponent, n, is determined solely by the nature of the inner surface of the pipe, while m depends on the distribution of the velocities throughout the cross-section, and on the temperature.

The fact that the reversed curves caused only about half as much increase of resistance as the continuous curves is certainly something to think about. It is to be hoped that further and more elaborate experimentation may bring out the laws which govern the resistances in various cases of flow in curves. It may not be out of place to suggest the bearing of such studies, not only on sinuous supply mains, but also on the flow in turbine wheels.

The results of the experiments on the hose when laid with easy curves and tangents are of interest in their relation to the results of the brass and galvanized pipes. The values of m and n, as obtained from a logarithmic plotting, are as follows:

Hose.	Diameter.	m, in feet per 100 ft.	n.
A	0.370 in	2.24	1.78
B	0.519 "	1.82	1.82
B_1	0.504 "	1.32	1.91
C	0.254 "	4.2	1.81

The experiments on B_1 were made with high velocities (7 to 22 ft. per second), and it is probable that the usual allowance for entry loss is insufficient, the velocities in the $\frac{3}{4}$-in. fittings being considerable. The high value of n and the low value of m bear out this statement. Hose B is free from this objection.

Especial attention is called to the low values of n for Hose A, B and C.

Experiments on Entry Loss.

The writers made some experiments to investigate the commonly accepted value for loss of head due to a square-edged entry. The

experiments were made on Pipes *II* and *X*. For Pipe *II* the cycloidal mouthpiece at the junction of the 5-in. brass pipe was replaced by a specially turned brass bushing with a plain inside face. The opening in this is 2.096 ins. in diameter, the same as Piezometers *A* and *B*. Section *A-B* of Pipe *II* was moved up stream next to the 5-in. pipe, and Piezometer *A* was inserted between the first two pipe lengths of the section. The slight ridge where the pipe was screwed up against a shoulder in the bushing was smoothed down. The entry length of Pipe *X*, likewise, was screwed into a brass bushing, giving a square-edged entry. An air-chamber was substituted for the end air-cock on top of the header, and the pressure was taken from this. The diameter of the header (2½ ins.) is so large, relatively to Pipe *X*, that practically static conditions prevailed. For Pipe *II*, however, the effect of the small velocity in the 5-in. pipe on the up-stream piezometer has been considered.

The results are given in Table No. 9. It will be seen that the values for Pipe *II* check fairly the accepted value, $0.5\frac{V^2}{2g}$. Pipe *X*, however, gives lower indications, the values seeming to average about $\frac{1}{3}\frac{V^2}{2g}$. These results indicate the possibility of a higher value than $0.5\frac{V^2}{2g}$ for pipes of commercial size.

Extent of Entry Disturbances in Succeeding Tangent.

The experiments on Pipe *II* seem to show that the piezometric effects of a square-edged contraction do not extend 78 diameters down stream. Section *A-B* gives normal results. In Pipe *X*, on the other hand, Experiments 666 to 670 show that 43 diameters are not sufficient, for both the high results for entry loss and the low results for the loss in Section *A-B* show that Piezometer *A* was in the abnormal-flow region. With these results the evidence obtained by the writers on the effects of curves* should be compared.

Practical Deductions.

The engineer will ask: "Of what use are refined laboratory experiments on pipes with conditions never met in practice?" Waiving the answer, for the present, let us examine the evidence to be had on the general subject of the flow of water in pipes.

* *Transactions*, Am. Soc. C. E., Vol. XLVII, p. 317.

TABLE No. 9.—ENTRY LOSS.

A.—PIPE *II.*

No. of experiment.	Temperature.	Observed head in water in section between Piezometer in 5-in. pipe to piezometer *A* in 2-in. pipe. Double centimeters.	Observed head in Section *A–B* in water. Double centimeters.	Loss of head due to entry, *A–B* considered normal. Double centimeters.	Velocity in Pipe *II*, in feet per second.	Remarks.
733	36.2°	22.89	39.92	$3.50 = 0.475 \frac{V^2}{2g}$	5.611	The up-stream section consists of 3.5 ft. of 5-in. pipe and 13.543 ft. of 2-in. pipe. Length *A–B* = 44.635 ft. Square-edged contraction. *A–B* is normal, since loss is the same as in other experiments.
734	36.0	16.85	29.95	2.48 = 0.454 "	4.811	
735	37.0	5.46	10.34	0.79 = 0.503 "	2.594	
736	36.8	10.61	19.40	1.50 = 0.453 "	3.761	

B.—PIPE *X.*

No. of experiment.	Temperature.	Observed head in Section Header–*A*. Double centimeters of mercury.	Observed loss of head in Section *A–B*. Double centimeters of mercury.	Loss due to entry alone, *A–B* considered normal in each case. Double centimeters of mercury.	$\frac{V^2}{2g}$ in double centimeters of mercury.	Velocity, in feet per second.	Loss in *A–B*, in feet per 100. Water.	Remarks.
660	54.5°	14.16	13.93	$0.71 = 0.333 \frac{V^2}{2g}$	2.13	10.628	170.7	In 660 to 665:
661	56.1	21.20	20.56	1.16 = 0.348 "	3.34	13.319	252.0	*l* (entry) = 5.459 ft.
662	57.8	8.50	8.48	0.40 = 0.330 "	1.21	8.011	103.9	*l* (*A–B*) = 6.719 "
663	47.8	8.60	8.64	0.40 = 0.339 "	1.18	7.924	105.9	
664	48.8	14.18	14.04	0.69 = 0.332 "	2.08	10.506	172.0	
665	50.9	5.38	5.47	0.23 = 0.324 "	0.71	6.136	67.0	In 666 to 670:
666	63.9	5.27	13.62	0.81 = 0.378 "	2.14	10.672	167.1	*l* (entry) = 1.144 ft.
667	46.2	5.17	13.94	0.76 = 0.372 "	2.04	10.407	170.8	*l* (*A–B*) = 6.719 "
668	48.0	3.63	9.93	0.54 = 0.386 "	1.40	8.611	121.7	
669	50.4	2.11	6.01	0.31 = 0.398 "	0.78	6.456	73.6	
670	52.0	7.94	20.38	1.22 = 0.376 "	3.25	13.150	249.7	
671	79.5	15.06	10.05	0.67 = 0.330 "	2.03	10.396	151.9	In 671 to 676:
672	82.9	22.63	14.89	1.09 = 0.339 "	3.22	13.076	225.2	*l* (entry) = 6.720 ft.
673	52.8	15.44	10.45	0.68 = 0.358 "	1.90	10.047	157.6	*l* (*A–B*) = 5.462 "
674	51.2	4.55	3.18	0.16 = 0.334 "	0.48	5.070	47.9	
675	51.8	9.26	6.34	0.38 = 0.352 "	1.08	7.588	95.6	
676	53.0	24.54	16.36	1.21 = 0.378 "	3.20	13.044	246.6	

Wherever possible, the original records of experimental data have been consulted. By the method of logarithmic plotting of losses of head with velocities, some 150 series of experiments have been investigated, and the values of m and n determined. The writers early came upon the necessity of either considering all the evidence, or else of running the risk of making errors in judgment and perhaps of allowing partiality more play than desirable in such a study.

In the preliminary plotting, all experiments which had been investigated were considered. In the final plotting many are left out. But the writers can state conscientiously that the conclusions are not altered thereby. The experiments omitted from the plotting are mostly old German and English experiments, the exact conditions and methods of measurement being generally unknown. If they were included (and the writers were sorely tempted to do so on account of the indications of most of them) they could not be given much weight, no matter what the indications, on account of this uncertainty.

Considering now the general subject, there is presented on Plate XI a logarithmic plotting of the values of m (as determined from plottings of each series of experiments), with the diameters of the pipes. It will be noticed that the values of n are marked near the points. These serve as descriptions of the nature of the pipe, from the experimental standpoint. In addition to this, the material is also indicated. This latter is the engineer's description, and is usually far from complete, but, with the assistance of the knowledge of the value of n, the indications can be discussed more rationally.

It will be seen that the range of pipe diameters varies from $\frac{1}{10}$ in. to 12 ft. A very casual examination will show that all the points lie in a comparatively narrow zone and hence indicate some exponential relation.

First of all, the line best fitting the points for the writers' brass pipes has been drawn. On account of the very considerable effect of temperature on the value of m, the middle points, for 55°, have been used. There are plotted, also, the points for 40° and 70°, but these are not used, except to convey to the eye the magnitude of temperature effect.

The variations in the value of n for the seamless-drawn brass pipes (all apparently of the same degree of roughness), have been noted. Hence, corresponding indications due to the values of m are to be

expected, and the variations from the line which has been drawn are seen on Plate XI.

One feature will be noticed at once. The line for the brass pipes forms the lower limit of the zone in which all the plotted points lie. This is another way of stating that these pipes represent the extreme of smoothness and ideal conditions. The equation of this line, neglecting the third place of decimals in the exponent, is

$$m = \frac{0.296}{d^{1.25}}$$

in which m is in feet per 1 000 ft., and d is in feet. Placing this value in the general expression, $H = m\ V^n$, and using the average value of n, there results

$$H = \frac{0.296}{d^{1.25}}\ V^{1.75}$$

in which H is in feet per 1 000 ft., and d is in feet. This expression, then, gives the loss of head for extremely smooth pipes laid straight. But this is not all. There occur maximum variations in the value of m of approximately 7% each way from the line of this equation. Hence, it is seen that this variation must be taken into account. There is also an effect due to the variation in n, and this may become considerable for high velocities. It is not as prominent a factor in the smooth brass pipes as in pipes used in practice, but it is pointed out as something to be borne in mind. The writers might wish, as many hydraulic engineers have wished, that there was less uncertainty in designing pipe systems, but, since it exists, its probable extent should be made an integral part of any formula. Therefore, concerning smooth brass pipes, under nearly ideal conditions, it becomes necessary to write, for the general expression,

$$H = \frac{0.296}{d^{1.25}}\ V^{1.75} \pm 7\%.$$

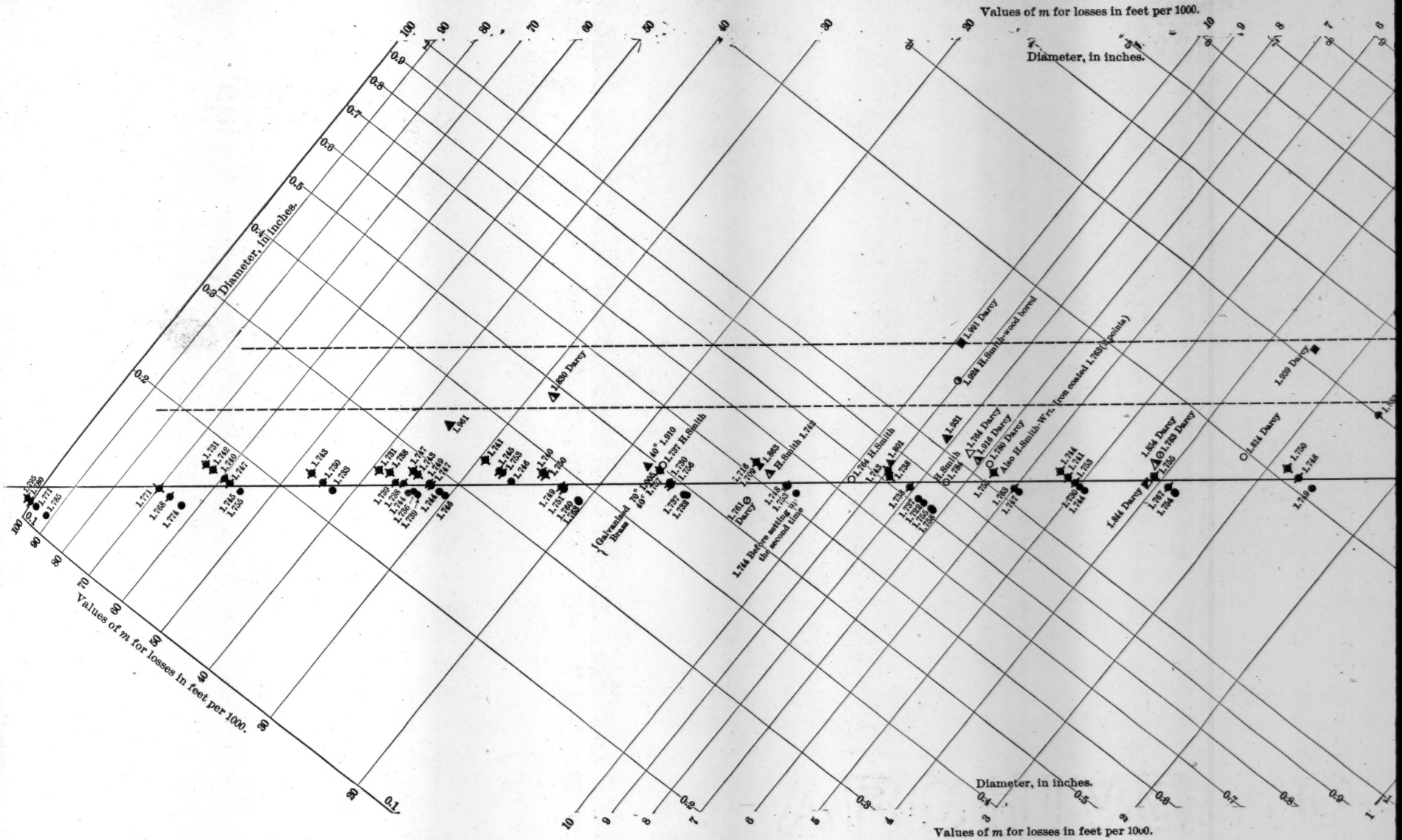
Looking at this, and remembering the long months of monotonous experimentation, the care taken to have everything accurate, the temperature factor, which alone may cause a variation as large as that given above, the writers feel that it nevertheless represents a step in advance; for while it does not tend to make hydraulic science any more exact, it at least helps to make it safer in application.

Modifying the above formula, there is obtained the expression

$$V = 104\ s^{0.57}\ d^{0.71} \pm 4\%;$$

or, using the "hydraulic mean radius,"

$$V = 279\ s^{0.57}\ R^{0.71} \pm 4\%.$$


Values of m for losses in feet per 1000.
Diameter, in inches.
Diameter, in inches.
Values of m for losses in feet per 1000.
Values of m for losses in feet per 1000.
Diameter, in inches.
1.891 Darcy
1.924 H.Smith-wood bored
1.859 Darcy
1.801
1.851
1.764 Darcy
1.916 Darcy
1.730 Darcy
Also H.Smith-Wrt. Iron coated 1.783(4 points)
1.659 Darcy
1.814 Darcy
1.654 Darcy
1.783 Darcy
1.844 Darcy
1.764 H.Smith
1.745
1.758
H.Smith
1.744
1.741
1.735
1.750
1.748
1.749
1.744
1.741
1.745
1.735
1.746
1.743
1.730
1.739
1.731
1.738
1.747
1.743
1.749
1.747
1.740
1.750
1.749
1.735
1.730
1.760
1.758
1.744 Before setting up the second time
1.761 Darcy
1.718
1.735
1.716
1.709
1.885
H.Smith 1.765
1.757
1.739
1.758
40° 1.910
1.797 H.Smith
70° 1.905
40° 1.765
Galvanized
Brass
1.771
1.768
1.771
1.731
1.748
1.740
1.747
1.745
1.735
1.743
1.739
1.735
1.731
1.738
1.747
1.743
1.749
1.747
1.744
1.746
1.741
1.745
1.735
1.746
1.740
1.750
1.749
1.735
1.730
1.760
1.758
1.744
1.741
1.735
1.730
1.744
1.783
1.735
1.767
1.754
1.744
1.741
1.735
1.750
1.748
1.749

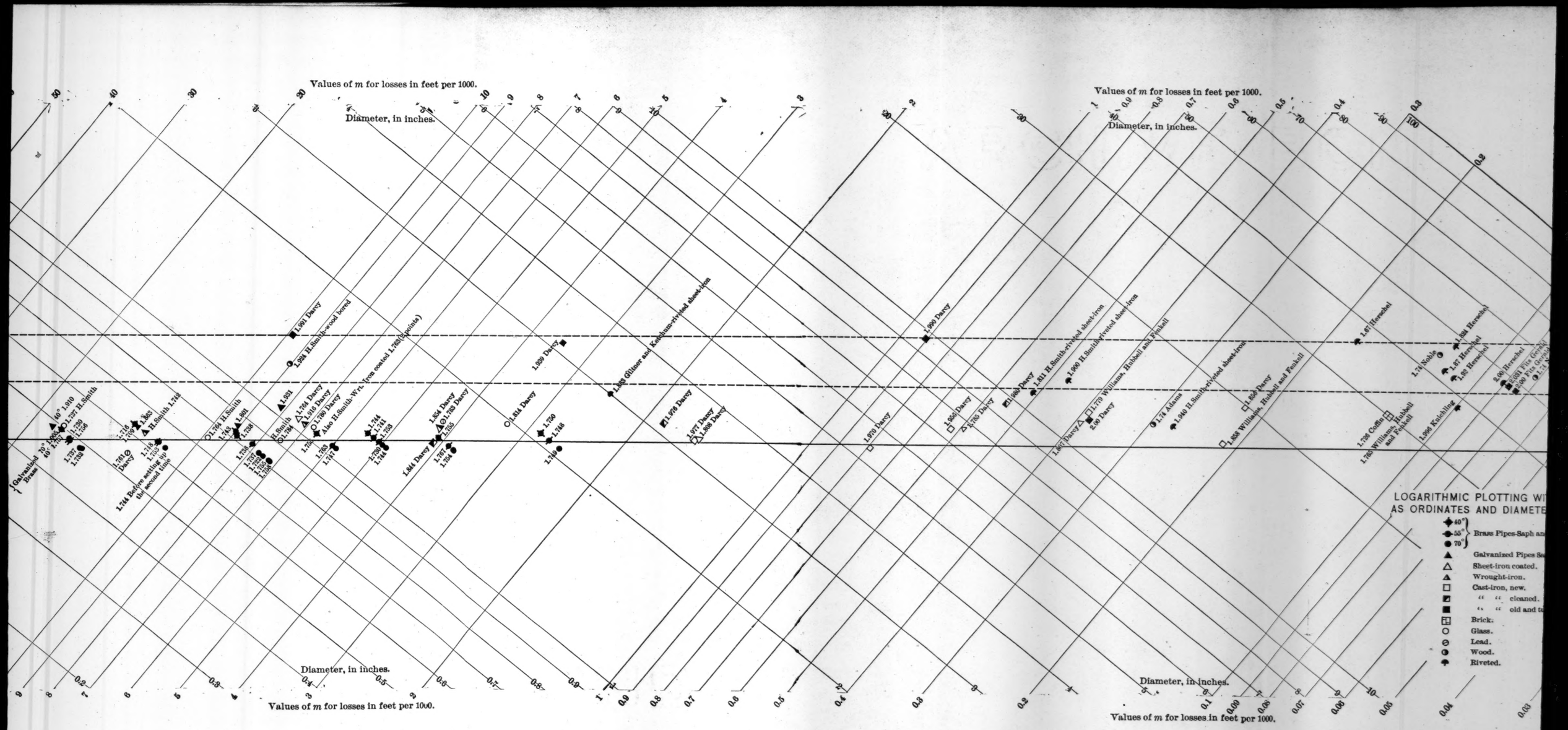
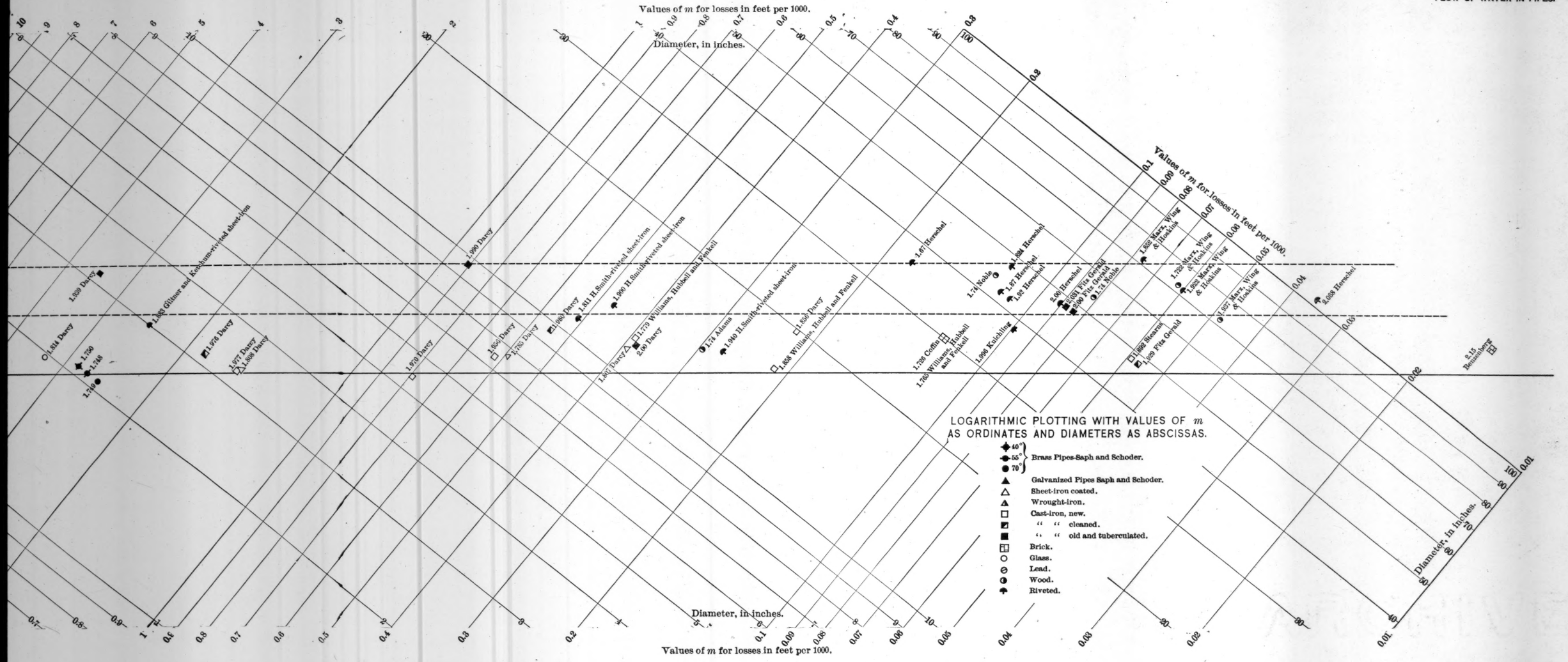

Values of m for losses in feet per 1000.
Diameter, in inches.
Values of m for losses in feet per 1000.
Diameter, in inches.
1.991 Darcy
1.924 H.Smith-wood bored
1.939 Darcy
1.900 Darcy
1.67 Herschel
1.84 Herschel
1.87 Herschel
1.88 Herschel
1.74 Noble
1.811 H.Smith-riveted sheet-iron
1.900 H.Smith-riveted sheet-iron
1.770 Williams, Hubbell and Fenkell
2.00 Darcy
1.74 Adams
1.940 H.Smith-riveted sheet-iron
1.838 Darcy
1.838 Williams, Hubbell and Fenkell
1.738 Coffin
1.765 Williams, Hubbell and Fenkell
1.966 Kuichling
1.764 H.Smith
1.764 Darcy
1.810 Darcy
1.760 Darcy
Also H.Smith-Wrt.-iron coated 1.765 (3 points)
1.744
1.741
1.735
1.854 Darcy
1.765 Darcy
1.844 Darcy
1.767
1.754
1.811 Darcy
1.759
1.748
1.719
1.978 Darcy
1.977 Darcy
1.868 Darcy
1.970 Darcy
1.866 Darcy
1.765 Darcy
1.980 Darcy
1.467 Darcy
1.744 Before setting up the second time
1.761 Darcy
1.718
1.735
1.737
1.739
1.738
1.910
1.787 H.Smith
1.883
H.Smith 1.749
1.716
1.709
Galvanized 0° 1.900
40° 1.910
Brass
Diameter, in inches.
Values of m for losses in feet per 1000.
Diameter, in inches.
Values of m for losses in feet per 1000.
LOGARITHMIC PLOTTING WI
AS ORDINATES AND DIAMETE
40°
55°
70°
Brass Pipes-Saph an
Galvanized Pipes Sa
Sheet-iron coated.
Wrought-iron.
Cast-iron, new.
" " cleaned.
" " old and ta
Brick.
Glass.
Lead.
Wood.
Riveted.

PLATE XI.
TRANS. AM. SOC. CIV. ENGRS.
VOL. LI, No. 964.
SAPH AND SCHODER ON
FLOW OF WATER IN PIPES.



In these last expressions, s is the hydraulic slope, in feet per foot of length. In these expressions, temperature effect has not been included. As has been stated, the effect is to vary the loss of head about 4% for a change of 10° Fahr. Owing, however, to the uncertainty regarding the effect on the rougher pipes used in practice, the writers feel that its insertion might be misleading.

Commercial Pipes.—Considering now the kinds of pipes used in practice, the writers desire, first of all, to call attention again to the indications of their experiments on galvanized-iron pipes. It is seen that, not only is there a variation in the exponent, n, from 1.80 to 1.96, but that Pipe *XVIII*, although having but two-thirds the internal sectional area of Pipe *XVII*, nevertheless, has a decidedly greater discharging capacity per unit of sectional area, or, in other words, offers less resistance to the flow, for the same velocity. Nevertheless, to the eye, all the pipes appear to be of the same degree of roughness. These two facts throw a strong illumination on the hitherto unaccountable variations in the results of standard experiments.

Although Darcy's experiments showed clearly the effect of variations in roughness which were easily perceptible, and which could be described so as to be recognized by ordinary inspection, yet the hydraulic literature of the past decade or two bears out the statement that the extent of the effects of variations in character of surface which practically defy detection have not been appreciated. Otherwise, how can we account for statements by well-known engineers that certain formulas will give results within 2 or 3% of the truth?

Turning again to the general plotting on Plate XI, we have to consider the experiments which lie in the zone, one of the limits of which is the line already discussed. Attention should be called to the fact that some of the series of experiments do not show much harmony in the indications, and that, therefore, it is a matter of judgment just where to draw the line on the logarithmic plotting of the losses of head and velocities. Hence, the values of m and n have the effects of the personal equation. But the two values, taken together, represent, as well as may be, the conditions in the range of the series under investigation.

It will be seen that the variations in the value of the exponent, n, are greater than the variations found by the writers for the galvanized pipes. Outside of the heavily tuberculated and riveted pipes, this

variation is from 1.74 to 2.00. The exponents given for the series by Herschel and by Darcy represent fairly the point which has been made. The values of m and n for the 30-in. pipe of Williams, Hubbell and Fenkell have been furnished to the writers by Professor Williams. They are the result of a plotting of the original data, on their own merits.*

It will be seen that a line parallel to the one already drawn will represent fairly the other limit of the zone. The experiments which determine this line are those by Darcy on tuberculated, cast-iron pipe, and some of those by Herschel, and some by Marx, Wing and Hoskins on riveted-steel pipe. Noble's 44-in. wood-stave pipe also comes close to this line. The equation is $m = \frac{0.687}{d^{1.25}}$. As for the general equation, we have a variation in the value of n from 1.67 to 1.99. The value, 1.67, from Herschel's 36-in. pipe, is lower than anything else on the plate. The writers consider that this value is questionable, although it is what the series of experiments gave.† Hence, the variation in n will be considered as from 1.82 to 1.99, and the general formula becomes

$$H = \frac{0.687}{d^{1.25}} V^{1.82 \text{ to } 1.99}$$

in which H is in feet per 1 000 ft., and d is in feet. Converting this into the other form, there appears

$$V = 54.7 \text{ to } 38.9 \, s^{0.55 \text{ to } 0.50} \, d^{0.69 \text{ to } 0.63}.$$

Using the hydraulic radius, this becomes

$$V = 142 \text{ to } 93 \, s^{0.55 \text{ to } 0.50} \, r^{0.69 \text{ to } 0.63}.$$

The two sets of equations thus far given, then, may be regarded as giving the two extremes, the first for very smooth pipes laid very straight, and the second for a considerable degree of tuberculation (or an inner surface comparable to it).

But neither one of these formulas represents the great majority of cases which confront the hydraulic engineer. These latter are included in the zone bounded by the full line and the middle line on Plate XI. The values of n vary from 1.74 to 2.00. The equation is $m = \frac{0.296 \text{ to } 0.469}{d^{1.25}}$ and the general expression becomes

$$H = \frac{0.296 \text{ to } 0.469}{d^{1.25}} V^{1.74 \text{ to } 2.00}.$$

*Not as described in *Transactions*, Am. Soc. C. E., Vol. XLVII, pp. 113 and 188.
†Mr. Herschel, himself, questions the reliability of seven out of eight experiments.

In the second form, this gives

$$V = 107 \text{ to } 46\, s^{0.57 \text{ to } 0.50}\, d^{0.72 \text{ to } 0.62},$$

or

$$V = 289 \text{ to } 109\, s^{0.57 \text{ to } 0.50}\, r^{0.72 \text{ to } 0.62}.$$

This last series of equations may be said to represent the range of results which the best modern experimenters have obtained on actual pipe lines in service or ready for service. The range of values is large, but the writers do not desire to narrow the analysis any more at present. Their purpose will have been accomplished if this paper serves to bring out some of the factors which have been neglected or not fully appreciated in the past. Above all, it is hoped that a vigorous discussion, in the true scientific spirit, may result, and that less of personality and more of the truth may appear than is wont to be the case.

The experiments, the data of which are given in this paper, were performed between February 24th, 1902, and February 25th, 1903. Experiments Nos. 25 to 771 were performed after July 10th, 1902. Earlier preliminary experiments, during 1901, are noted in the paper.

The original notes and the data tabulated from them are on file in the Hydraulic Laboratory of the College of Civil Engineering of Cornell University, where they are available for examination at any time.

APPENDIX.

Discussion of Theories.

The writers do not venture, at present, to give any hypothesis showing why the exponent for smooth pipes should be 1.75. They merely give it as an experimental fact.

There has been an attempt, however, to explain the matter by saying that the resistance is really composed of two parts, one due to the true friction on the pipe wall, and depending on the first power of the velocity, and the other due to the impacts of the particles on each other and depending on the square of the velocity. The theory states that the composition of these two parts will be equivalent to one term with the power, 1.75. The formula which expresses this idea is $H_f = xV + yV^2$, x and y being coefficients, the values of which are to be determined. Since this formula (or its many modifications) has been used extensively, and since it expresses the idea put forth above, the writers have investigated it, to see, if in this form or in any of its modified forms, it can be made to represent accurately a series of experimental results.

In order to determine the values of the coefficients x and y, at least two methods have been in vogue. The first—the method of least squares—is allowable only on condition that the coefficients are proved to be constants, otherwise the formula derived and its limits of error apply only to those experiments which are used, and its application may be limited. The second method—a graphical one—is for this reason more general, but in most cases in which it has been applied it has not been carried to completion. It consists in obtaining first a series of equations in x and y by substituting the observed values of H_f and V. These equations may be represented by straight lines, and, if x and y are constants, all these lines should pass through one point the coördinates of which are the desired values. Ordinarily, however, it is said that the lines will not pass through a point, but will enclose a small area, and in such a case the coördinates of the center of gravity of the area are the values of x and y. This method has been applied by the writers to a number of series of experiments, and in no case, if a suitable scale were used, did the lines pass through a point or enclose an area. In every case the lines formed a series of tangents to a regular curve similar to one branch of a hyperbola, and to which the axes were apparently asymptotic. This showed clearly that x and y, instead of being constants, were really functions of V. Further investigation also showed that x and y were always such functions of V that the final result was that obtained by a direct logarithmic plotting.

For example, if the logarithmic plotting leads to $H_f = m\ V^{1.75}$, this method leads to $H_f = x\ V + y\ V^2 = (a\ V^{0.75})\ V + (b\ V^{-0.25})\ V^2 = (a + b)\ V^{1.75}$. If it is admitted that $H_f = m\ V^{1.75}$ represents accurately the relation, it can also be proved mathematically that any attempt to express the relation in two terms will lead to variable coefficients, and finally result in the same way as shown above.

A more general expression, $H_f = a\ V + b\ V^2 + c$, is sometimes used. It can be shown, also, that a, b and c will be functions of V. In a number of cases where this form has been used, negative values of c have resulted when a, b and c have been found by least squares. These, surely, are not easily interpreted.

The foregoing investigation shows that the explanation given for the presence of the exponent, 1.75, and which is expressed by the Prony formula, $H_f = x\ V + y\ V^2$, by those who hold to this idea, is questionable. It also shows that all formulas which involve V^2 in any way will also involve in general a coefficient depending on V. The exception consists in the fact that a very few experiments on pipes do show an exponent of 2. As is readily seen, we thus account for the variables, f and c, in the familiar formulas, $H = 4f\frac{l}{d}\frac{V^2}{2g}$ and $V = C\sqrt{r\,s}$. Of all the forms yet advanced, the only one which will represent the conditions at all accurately is the one the writers have adopted, $H_f = m\ V^n$, and n is to be determined for each series of experiments. It will be seen, of course, that the formula, $V = C\,r^x\,s^y$, is merely a modification of $H_f = m\ V^n$, m being dependent on d.

The writers, for a time, had the idea that the reason for the exponent of V being different from 2 might lie in not distinguishing between the square of the mean velocity and the sum of the squares of the individual velocities throughout the cross-section of the pipe. (They fully recognized, however, the fact that the idea of flow in parallel filaments is merely a theoretical convenience.) They therefore have investigated the question as to what power of V_m will give the same result as $\frac{\Sigma\ V^2}{\pi\ r^2}$. The supposition that the velocity curve is an ellipse, led to results which indicated that their view was correct. The exponent of V_m had a value of 2 for a velocity of 8 ft. per second, and decreased for lower velocities. For a velocity of 1 ft. per second, many peculiarities in the values of the exponent were shown similar to those which the present experiments show in the critical-velocity transition stage. However, the best test of theory is experiment, and when a series of Pitot tube traverses was investigated, there was found the experimental fact that the relation, $V_m^2 = \frac{\Sigma\ V^2}{\pi\ r^2}$, holds almost exactly in every case. While proving that the proposed explanation was

incorrect, this fact will doubtless be of value to those who are interested in determining the true form of the velocity curve.

In connection with the value, 1.75, obtained for the exponent on brass pipes, it will doubtless be of interest to mention the fact that Hagen gives the following relation for obtaining the volume of a unit weight of water at any temperature:

$$(t - x)^{1.743} = n\,(G - y)$$

where t is the temperature, x the temperature at the greatest density, G the volume of a unit weight at the temperature t, and y that at the temperature x. The writers would not venture to say that there is any connection between this exponent, 1.743, and the exponent which they obtain for smooth pipes. The coincidence, however, is interesting.

DISCUSSION.

A. FLAMANT, Esq.* (by letter).—This paper is exceedingly interesting. For brass pipes of small diameter, the experiments confirm the law of variation of resistance proportionate to the power, 1.75, of the mean velocity, the law which the writer applied in 1892† to pipes of cast iron in ordinary use, and for which he has drawn up numerical tables, etc. Mr. Flamant.

But, on the other hand, the authors find, for galvanized pipes, a variation proportionate to the power, 1.90 (about), of the velocity; which seems to indicate that the exponent of V varies with the condition of the interior surface of the pipes.

From this point of view, it would be interesting to make new experiments with pipes of different materials, such as glass, earthenware, wood, wrought iron, cast iron; or, perhaps more simply, to ascertain the effect of an interior coating of paint, tar or some similar material. Perhaps, by making a sufficient number of experiments, comparative among themselves, one could formulate a definition or a more exact indication of what is called the rugosity, and which, according to the authors, seems to influence not only the coefficient, m, of their formula, but also the exponent, n.

As to the effect of temperature, the writer believes that it is especially appreciable because the diameters of the pipes experimented upon are small. This effect ought to diminish rapidly, as the diameter increases, and, for the usual diameters, should become entirely negligible. This, at least, seems to have been established by M. V. Fournié, in an article‡ from which the conclusions are quoted by the writer on page 144 of the second edition of his work on "Hydraulics" (Paris, 1900).

Although it was known, the confirmation of the increase of flow, with the temperature, for pipes of small diameter, is interesting. The writer is surprised, therefore, that this variation has been found so nearly the same for the very small diameters (as of Pipes *XIV*, *XV* and *XVI*), as for the larger (as Pipes *II*, *III* and *IV*). It would be useful to ascertain whether the same effect would continue to appear in pipes of the usual diameter, 20 cm. to 1 m., for example.

On the whole, the work of Messrs. Saph and Schoder has brought out a valuable collection of precise, experimental facts which will be very useful for the solution of this difficult problem, and which seem to be especially of a nature to solve new questions of detail, which makes it desirable that these capable observers should continue their researches.

* *Ingénieur des Ponts et Chaussées.*
† *Annales des Ponts et Chaussées*, September, 1892.
‡ *Annales des Ponts et Chaussées*, 1898, 3e Trimestre.

Mr. Mills. HIRAM F. MILLS, Esq. (by letter).—The writer has examined with a good deal of care most of the experimental results presented by Messrs. Saph and Schoder, and must express his cordial appreciation of the care and skill with which this series of experiments has been planned and executed. The educational institution that prepares men for and fosters such work is to be highly commended.

The data furnished by the observations are a valuable addition to the science of hydraulics, and worthy of the careful study of its students. They give a distinct addition to our knowledge of the effect of temperature upon the flow of water in pipes, and it is desirable that still further knowledge be obtained upon this subject, for, while these experiments upon small smooth pipes bring out the effect in a very marked degree, the effect is not to be neglected in larger pipes used in water-works.

The most interesting single series of experiments, because it gives the slopes for both high and low temperatures through the greatest range of velocities, is that upon the smallest brass pipe, which is about $\frac{1}{10}$ in. in diameter. These experiments illustrate conditions which are met in hydraulic experiments with larger pipes in a much smaller degree, and serve to explain some apparent anomalies therein.

With temperatures of water near 70° Fahr., the slope varies nearly with the first power of the velocity, being contained between $S = 0.129\ V$ and $S = 0.138\ V$, for all velocities given up to $V = 2.309$ ft. per second. Above this velocity the slope increases much more rapidly, changing from $V = 2.559$, with $S = 0.3736$ per foot, to $V = 2.917$, with $S = 0.6061$ per foot, which is an increase of slope with the 3.69 power of the velocity. Upon reaching a velocity of 3.3 ft. per second, the slopes come into line with those of higher velocities which vary with a power of the velocity somewhat less than the second power, being, with this very smooth pipe, near the 1.75 power. With small velocities, with slope increasing directly with the velocity, the water having the lower temperature has the greater slope, but, during the rapid increase that follows, the warmer water, being less viscous and more mobile, makes this change at smaller velocities than the colder water, and the slope for 75° is greater than the slope for 70°, and both become greater than that for 38.5 degrees.

With the temperature of the water at 38.5°, the slope increases with the first power of the velocity at a much more rapid rate than with a temperature of 70°, and continues in this condition through much higher velocities. Its slope is expressed by $S = 0.212\ V$, or 52% greater than water at 70°, and continues at this rate until it reaches a velocity of about 3.5 ft. per second. At a velocity of about 2.9 ft. the warmer water, in its rapid increase of slope, reaches the slope of the colder water and soon exceeds it, while the colder water, keeping on with its steady rise, gets to have a slope 14% less than the slope of water at 70° at the same velocity.

At a velocity of about 3.5 ft. the water at 38.5° begins to change its condition, and its slope increases with about the 2.5 power of its velocity and equals the slope of water at 70° at a velocity of 4.5 ft. per second, then rises above it, and, at 5.4 ft. velocity, reaches its permanent condition of flow for higher velocities, and continues with about 10% more slope than the water at 70° temperature. Mr. Mills.

It is interesting to compare this result with experiments which the writer made, in 1875 and following years, upon the flow of water through a cast-iron, tar-coated pipe, 1 ft. in diameter, which showed about 4% increase in slope with decrease in temperature through the same range.

But, in order to understand the effect of temperature, and study its laws, the writer thinks it necessary to divide the slope into its two parts, one depending upon shocks and mass, and varying with the square of the velocity; and the other depending upon cohesion and adhesion, and varying with the first power of the velocity.

Making such division, as well as he may from the experiments, the writer finds that, in the small brass pipe, the part of the slope varying with the square of the velocity increases about 8% with the decrease in temperature, and the part varying with the first power increases about 17%, while in the large pipe having a diameter 120 times as great, the part varying with the square of the velocity increased not more than 2%, and the part varying with the first power of the velocity increased as much as 29% with the decrease in temperature from 70 to 38.5 degrees. Much more may be deduced from these experiments to show that the principal effect of temperature is upon that resistance to motion due to cohesion and adhesion which varies with the first power of the velocity.

Edgar C. Thrupp, Assoc. M. Inst. C. E. (by letter).—The writer shares the authors' desire to abolish the term, $\frac{V^2}{2g}$, from pipe-discharge formulas, although their illustration of water being forced through a pipe by compressed air is not a very happy argument against it. The faith of students will be severely strained if they are taught to believe in the theory when abundance of experimental evidence exists showing the index of V to vary from 1 to 4. A more satisfactory view is to regard a formula for the flow in pipes as a statement of the conditions under which the frictional resistance equals the moving force. Gravity undoubtedly determines the moving force, but has nothing to do with the frictional resistance. Gravity sets the water in motion and increases its velocity until its force is balanced by friction, but this is a very different thing from determining the velocity as proportional to $\frac{V^2}{2g}$. Mr. Thrupp.

The writer does not agree with the authors' assumption of the continuity of their law of critical velocities $\left(V = \frac{0.088}{d^{0.784}}\right)$ for pipes of

Mr. Thrupp. larger diameter, as he knows from his own experiments that there is a rapid divergence from such a law when the hydraulic radius exceeds $1\frac{1}{2}$ ins., and that the critical velocity then increases with the hydraulic radius. The question of the critical velocity may be of little importance as regards the water supply of towns, but it will be found to be a matter of very great importance in irrigation and estuary or tidal-river hydraulics, particularly in relation to silt-carrying and scouring, and also in connection with the resistance of high-speed ships.

The authors seem to be unduly confident in their assumption that the index of R may be taken as constant through a wide range in values of R, in the formula

$$V = 279\, S^{0.57}\, R^{0.71} \pm 4 \text{ per cent.}$$

The writer investigated this point very fully in 1887, and arrived at the conclusion that for small values of R the index must be greater than for large values, and gave an empirical method of expressing the variation.* The authors' experiments confirm the writer's conclusion because they show an index of 0.71 for small, smooth pipes, and this is certainly too large for pipes more than 6 ins. in diameter.

The region covered by the experiments has now been well explored, and there is not much to be learned by further experiments on small pipes, but if some of the technical colleges having hydraulic experiment facilities were to use them for research on the critical-velocity phenomena in relation to R and V, and the hydraulic gradient in open channels with $R = 2$ ins. and upward, much information of practical and scientific value would be brought to light.

Mr. Hazen. ALLEN HAZEN, M. Am. Soc. C. E. (by letter).—The paper records a most interesting investigation of the flow of water in small pipes.

There seem to be two elements in the friction of water in small pipes: One depends upon the viscosity of the water, and is of controlling importance in capillary tubes; the other seems to depend more upon the kinetic energy required to produce the motion of the water in whirls and eddies and other motions, and this element is of controlling importance in larger pipes. It might naturally be supposed that each of these elements of friction would be present in all cases, but in varying relative amounts. Darcy's formula for the flow of water in pipes is in a form corresponding to this idea; that is to say, two terms are used, one, varying with the first power of the velocity, which is the power which applies in capillary tubes; and a second, varying with the square of the velocity, which is the power found to apply to the flow of water through very rough and crooked channels of large size.

The experiments of the authors do not indicate such a gradual transition from one condition of the flow to the other. They indicate, rather, that the flow of water in tubes, up to a certain point, called the critical velocity, follows accurately the flow of water in capillary tubes,

* *Transactions* of the Society of Engineers, 1887.

and without change in the exponents and coefficients; and that from a point slightly above this critical velocity the laws of flow are the same as for much larger pipes, and that the same exponents and coefficients apply. Mr. Hazen.

The writer's experience with the flow of water in gravels had led him to anticipate a more gradual change than that shown by the authors. For a gravel at low velocities the friction varies with the first power of the velocity, and, as the velocity increases, the divergence from this law is at first slight and gradually increases, until finally, at much higher velocities, it varies with the square of the velocity.

The experiments of the authors are most useful in throwing light upon this point. The change from one state of flow to the other may be compared to the flow of water through a vertical orifice. With water slowly rising behind it, the orifice first acts as a weir. It follows precisely the law of discharge over a weir until the water hits the top of the orifice. Thereupon the flow is at once retarded, and it commences to act as an orifice, and follows perfectly the law of discharge through an orifice. There is no intermediate ground. It acts either as a weir or as an orifice. But there is a certain range in which it will act as an orifice, if it happens to be started that way, or as a weir, if it happens to be started that way, and it will not change from one to the other except by some outside influence, and it may be somewhat the same with small pipes.

The authors have discussed at length the formula for the flow of water in brass pipes above the critical velocity. The experiments also furnish some data for computing the flow below this velocity, but this matter is not discussed in the paper. The writer has taken the results of those of the experiments which seem to be clearly below the critical velocity, and has compared them with the formula for the flow of water in capillary tubes, which is taken to be

$$v = c\, s\, d^2 \left(\frac{t+10}{60}\right);$$

c being a coefficient; s the slope; d the diameter, in inches; and t the temperature, in degrees, Fahrenheit.

The factor $\left(\frac{t+10}{60}\right)$ is one which was deduced from experiments at Lawrence to represent the flow of water, at different temperatures, through capillary tubes. It also represents the relative rates at which water passes through sand, and at which silt settles through quiet water. This term is more convenient for practical use than the conventional factor of Poiseuille, and is sufficiently accurate. It seems to be based on one of the primary physical properties of water, and is applicable directly, and with considerable accuracy, to all the phenomena which depend upon the viscosity of water.

Mr. Hazen.

TABLE No. 10.

No. of experiment.	No. of pipe.	Diameter of pipe, in inches.	Slope, in feet per hundred.	Velocity, in feet per second.	Value of c, temperature factor omitted.	Temperature.	Reciprocal of temperature factor.	Value of c, corrected for temperature.
201	*VII*	0.631	0.087	0.218	630	71.8	0.732	462
200	.....	0.631	0.187	0.471	630	71.3	0.739	465
292	*IX*	0.376	0.125	0.111	628	69.0	0.760	477
291	.....	0.376	0.320	0.291	641	69.0	0.760	487
290	.....	0.376	0.590	0.541	648	68.8	0.761	493
289	.....	0.376	0.955	0.849	628	68.6	0.765	479
298	*XIII*	0.221	0.870	0.267	628	62.4	0.829	520
297	.....	0.221	1.860	0.540	592	62.1	0.832	493
296	.....	0.221	3.140	0.898	582	62.0	0.833	485
294	.....	0.221	4.470	1.270	580	61.5	0.839	487
98	*XV*	0.151	2.001	0.300	658	70.0	0.750	493
97	.....	0.151	8.623	1.248	634	70.0	0.750	475
230	.....	0.151	9.330	1.362	640	73.1	0.721	462
258	.....	0.151	11.470	1.767	675	72.9	0.723	489
114	*XVI*	0.107	5.910	0.451	665	70.9	0.740	492
102	.....	0.107	7.804	0.602	672	69.5	0.755	507
113	.....	0.107	11.710	0.911	680	70.4	0.745	508
101	.....	0.107	18.290	1.378	655	69.0	0.758	497
108	.....	0.107	23.600	1.790	660	71.9	0.732	483
100	.....	0.107	28.590	2.074	635	68.0	0.768	488
112	.....	0.107	31.930	2.309	630	70.2	0.748	472
221	.....	0.107	8.970	0.730	710	75.2	0.704	500
220	.....	0.107	19.890	1.614	708	75.1	0.703	498
442	*XVI*	0.107	7.200	0.387	470	38.3	1.240	584
441	.....	0.107	19.000	0.958	440	38.4	1.240	545
440	.....	0.107	33.300	1.635	428	38.5	1.240	531
439	.....	0.107	65.800	3.142	416	38.5	1.240	515

In Table No. 10, the writer has stated the experimental results, as taken from the authors' tables, and has computed the value of c, first, without using the temperature factor, and afterward, the temperature factor appears, and finally, the value of c, corrected for the temperature factor.

The changes in temperature in these experiments were too slight to throw much light on the application of the temperature factor. Only in the case of the smallest pipe, No. *XVI*, was there a considerable range in the temperature. This series indicates a variation in flow with change in temperature amounting to about 79% of that computed in the formula stated above. The writer thinks there can be no question that the temperature factor will apply much more closely than this to small pipe. Possibly the smaller variation with this pipe indicates some change in the condition of the pipe between the two series of experiments, or it may indicate the beginning of a gradual change in the form of flow and a slight tendency to the condition with higher velocities, where the influence of temperature is comparatively slight.

It is interesting here to note that the experiments of Hagen, as quoted by the authors, seem to correspond very closely with the

above-mentioned temperature factor. Thus, taking the smaller pipe, 0.00924 ft. in diameter, or approximately 0.11 in., at a slope of 1 in 10, the temperatures, velocities, etc., are as given in Table No. 11. Mr. Hazen.

TABLE No. 11.

Temperature.	Velocity, scaled from plotting.	Reciprocal of temperature factor.	Product.
41	0.451	1.176	530
47.3	0.506	1.045	530
50.9	0.547	0.986	540
54.7	0.581	0.926	540
66	0.675	0.790	533
75	0.770	0.705	544
80	0.832	0.667	555
88	0.900	0.611	551
100	0.955	0.545	552
110	1.09	0.500	545

The product is constant, and the agreement is as close as the velocities can be scaled from the plotting.

The experiments of the authors indicate that c, in the above-mentioned formula, ranges from 462 to 584, averaging about 495. The experiments of Hagen show a value about 10% lower. The experiments thus serve to establish a coefficient in the formula for the flow of water in small pipes below the critical velocity, and the data are most useful in this direction.

An interesting deduction is that the critical velocity in a pipe is at a point where the flows computed by the formula for the flow of water in larger pipes and by the formula for the flow in capillary tubes are equal, and that in all other cases the flow follows the formula which gives the lowest velocity for a given slope.

The effect of temperature upon the flow of water in small pipes above the critical velocity is most interesting. The effect of temperature seems to be less than one-quarter of what it is below the critical velocity. It is, nevertheless, large enough to be an important element. The question may be raised as to whether temperature exerts a corresponding influence in much larger pipes, or whether the effect noted in these experiments represents the beginning of a gradual change toward the capillary condition. The experiments themselves give no indication of this, as the effect of temperature seems to be quite as great with the larger tubes as with the smaller ones, but, if it should prove to be otherwise, it would affect materially some of the deductions of the authors.

The general form of formula proposed, although very similar to some of the formulas which have been used, presents some interesting points. According to the authors, in the general formula,

$$v = c\,(r^x\, s^y),$$

x and y are proportional to each other, and x is always 1.25 of y. This

Mr. Hazen. conclusion is diametrically opposed to that of Tutton,* whose analyses led him to the conclusion that, as x increases, y decreases, and that the sum of the two exponents is a constant, namely 1.17.

It is comparatively easy to determine the value of y in particular cases. It involves passing water through a pipe at different velocities and noting the heads required. This is an experiment for which there are many opportunities, and sufficient data are available to give a very good idea of the value of this exponent for various conditions. It is much more difficult to find the value of x.

The authors have a very simple method of computing x when y is known. Tutton's method is equally easy, but, unfortunately, leads to very different results. It has seemed to the writer that the most accurate value for x could be secured by comparing the results obtained for very small and very large pipes. Of course, it is impossible to secure very large pipes with precisely the same kind of interior surface as obtained in very small pipes, but it seems safer to compare the results obtained from very large and very small pipes, even though their interior surfaces do differ somewhat in character, than to take the indications of experiments more closely comparable, but covering a shorter range. These experiments afford some admirable material for use in this way, but it may be questioned whether the method of computation is the best that could be used.

The proposition that the friction varies inversely as the 1.25 power of the diameter is derived from the plotting reproduced in Plate XI. This plotting gives the values of m, in comparison with the pipe diameters, in inches. An examination of the formula used by the authors shows that this value of m is the slope in feet per thousand when the velocity is 1.00. This value is usually not the result of direct observation, but is computed from observations taken at much higher velocities.

The exponents shown by the authors, in connection with the various experiments, indicate the rate at which the slope increases with increase in velocity. At that end of the plotting which shows the smallest pipe, these exponents average, as stated by the authors, about 1.75. At the opposite end of the plotting the exponents average much higher than this. Beginning at the upper end and keeping near the lower line, indicating smooth pipe, there are positions marked: Benzenberg, 2.15; FitzGerald, 1.929; Stearns, 1.892; Williams, Hubbell & Fenkell, 1.765; Coffin, 1.726; Williams, Hubbell & Fenkell, 1.858; Hamilton Smith, 1.94; Adams, 1.74; Darcy, 2.00; Williams, Hubbell & Fenkell, 1.779; Darcy, 1.807. The average of these exponents is 1.87.

The fact that the exponents are higher at this end of the line than at the other indicates that if a comparison were made at a higher

* *Journal of the Association of Engineering Societies*, Vol. XXIII, p. 151 (1889).

velocity than that assumed, which is 1 ft. per second, the slopes would be increased more for the large diameters than for the smaller ones. In other words, the ratio between the exponents of r and s would be reduced. Mr. Hazen.

Plotting the results of the experiments relied upon by the authors for a velocity of 3 ft. per second, and using the actual experimental values for the velocities, or values reduced from the experiments nearest to them, it is found that they diverge much more from the line for the authors' experiments for brass pipe than is the case for the plotting of the values of m. It is, unfortunately, a fact that probably none of the large pipe had as smooth surfaces as the brass pipe used by the authors, and some allowance must be made for this fact in the comparisons; and, while the line drawn as a general average would be a matter of individual judgment, the writer would hardly give it an inclination greater than that corresponding to an exponent of 1.20. If the matter were carried further, the exponent would probably be lower. Further, if the temperature effect should be found to be different in large and small pipes, the value would vary with the temperature.

For these reasons, care must be taken in extending the values found for very small pipes to large sizes, and it would be better to go slowly in accepting a constant ratio between the exponents of the general formula.

The experiments are of no less value because of this. A critical examination of extremes may lead to a more accurate understanding of intermediate values, and in this case the large number of experiments exceeds anything available for the larger sizes of pipe for which accurate computations are most often desired.

It seems that every discussion of this subject only makes it clearer that no one formula can be made to apply to all the conditions that there are, and, fortunately, such a formula is not necessary for practical purposes. The Chezy formula is most useful, notwithstanding its wide divergences from ordinary conditions. It is easy to devise exponental formulas to represent the average of particular experiments, but the multiplication of formulas is confusing. The writer has felt, however, that a formula with exponents representing more nearly than those of the Chezy formula the average of all-round water-works practice, with such values of c as should be found by experience, would be serviceable, particularly if an easy means of solution of problems by it were provided. This idea was taken up with Professor Gardner S. Williams, M. Am. Soc. C. E., and the formula,

$$v = c\, r^{0.63}\, s^{0.54}\, 0.001\, (^{-0.04})$$

was adopted, not as representing clean pipe or dirty pipe, or large pipe or small pipe, particularly, but simply as an approximation to the conditions most commonly met, particularly in water-works prac-

Mr. Hazen. tice. To solve this formula, a slide rule was designed, which was first constructed in a home-made way, and afterward more accurately by an instrument maker. This rule allows a very rapid solution of common hydraulic problems by the formula, and the regularity in the values of c found applicable with it indicates a sufficiently close approximation to accuracy in the exponents selected, for most practical purposes.

Mr. Coker. E. G. COKER,* Esq. (by letter).—The authors, in commencing their investigations, do not appear to have given much attention to the important researches of Osborne Reynolds on the flow of water in pipes, now twenty years old. He first showed that water flowing in a pipe may be direct or sinuous, depending upon the relations between the velocity of the water, the temperature, and the diameter of the pipe; and he gave a formula, for the critical value of direct or stream-line motion, expressed in the form,

$$v_c = k\, D^{-1} f(T),$$

where v_c is the critical velocity, D is the diameter of the pipe, and $f(T)$ is $(1 + \alpha T + \beta T^2)^{-1}$.

He showed, further, that, for stream-line motion, the loss of head is proportional to the first power of the velocity, and is influenced greatly by temperature; while, for eddy motion, the resistance is proportional to a higher power of the velocity, and is less dependent on the temperature. These results were arrived at by a train of reasoning which appeals to the principle of the equality of dimensions of physical quantities, and the deductions were amply verified by his own experiments and others. It is somewhat surprising, therefore, to see some of these conclusions stated as if they were newly discovered and therefore novel, whereas they have long since been accepted and find a place in modern textbooks.

Again, the authors express astonishment in finding that the values of n, in the formula, $H = m\, V^n$, is remarkably constant for pipes of the same material and smoothness, but this need hardly be a matter of surprise when it is remembered that, even for pipes of widely different materials and of different degrees of roughness, the value of n, in the great majority of cases, lies between 1.7 and 1.95, and there are many published experiments giving logarithmic plots for pipes of the same material and degree of roughness with lines practically parallel.

Most of the experiments recorded in the paper have been made for eddy motion in which a small difference of temperature between the water in the pipes and the laboratory does not cause serious error, but this is not the case when there is stream-line motion, and it is then very necessary to guard against a change of temperature in the flowing water by lagging or otherwise protecting the pipes. The tempera-

* Assistant Professor of Civil Engineering, McGill University.

ture of the room where the experiments were carried out is stated to have been practically constant, but, apparently, there is no reference to the pipes being protected from change of temperature, and it would be interesting to know whether this precaution was attended to. Mr. Coker.

The method of reducing the results to a standard temperature appears to be open to criticism in that the corrections are based entirely on the results, and hence any errors in the experiments reappear in the correction factors. If this objection be removed it would be interesting to learn why the authors finally assumed a uniform correction of 4% per 10°, when, as appears from Fig. 3, the correction should vary from 2.9 to 4.6 per cent.

If it is assumed that the total resistance, h, depends on powers of the pipe diameter, the viscosity, the density, ρ, and the velocity of the water, there is no difficulty in determining a correction factor for variations of temperature, t, in the form

$$\frac{d\,h}{h} = (n-2)\,\frac{\alpha + 2\,\beta\,t}{1+\alpha\,t+\beta\,t^2}\,d\,t + (1-n)\,\frac{\rho}{1-\rho\,t}\,d\,t,$$

where the second term, involving the density, ρ, is, in general, negligible. This correction factor is independent of the experiments, except as regards n, which latter can always be determined with considerable accuracy, while the constants, α and β, relate to the known variation in the viscosity.

George H. Fenkell, Assoc. M. Am. Soc. C. E. (by letter).—As the title of this paper does not seem to limit the discussion to experiments upon the smaller sizes of pipes, such as those used by the authors, the writer will give the result of a few experiments which he made recently on the loss of head in 8 215 ft. of 60-in. cast-iron intake pipe at Erie, Pa. Mr. Fenkell.

In 1895 and 1896 the Commissioners of Water-Works of Erie, Pa., laid a cast-iron intake pipe from the pumping station on the south shore of Erie Harbor to within ½ mile of the north shore of the harbor. The top of this pipe was laid about 3 ft. below the natural bottom of the harbor, and the outer end provided with a submerged intake crib. As the manner of laying this pipe has been described*, it is sufficient to say that the pipe was coated with tar, and that sections 72 ft. 10 ins. long were made up on shore, launched, towed into position, sunk, and connected by divers, and that when the line was completed it was tested thoroughly for leaks. A profile of the line, made after the work was completed, shows that there is very little variation in the vertical alignment, except where the pipe approaches the surface, near the shore end.

The water passes through the gratings in the top of the crib, then through 6 ft. of 60-in. vertical pipe to a 60 x 60-in. tee, where it is deflected horizontally into the main line, whence it is conducted to the

* *Engineering News*, Dec. 5th, 1895.

Mr. Fenkell. pumping station. The observations for loss of head were made in a 24-in. manhole in the water-works park near the pumping station, and between the crib and this manhole the only special is a 60-in., 35° curve. The top of the 6-ft. vertical pipe at the crib is provided with a 5-in. flange, and is about 18 ins. above the crib floor. By feeling through the ice with a piece of gas pipe, during the winter of 1902–03, the top of the crib seemed to be free from sticks and leaves, and no mud was found, either in the bottom of the crib or in the inside bottom of the 60 x 60-in. tee. No deposit could be felt at the 24-in. manhole, and the 24-in. riser at the manhole, which was set when the pipe was laid, seemed to be free from rust, although in places it was somewhat rough.

Erie Harbor is about 5 miles long by nearly 2 miles wide, and its greatest depth is about 29 ft. The water at the crib is about 23 ft. deep. As the passage from the harbor into Lake Erie is only about 400 ft. wide, storms never cause the water in the harbor to become very turbid.

The experiments were made on September 1st, 1903. The minimum temperature of the air during the previous night was 62° Fahr., and the mean temperature of the air when the observations were made was 68° Fahr. The temperature of the water was 66° Fahr.

The day was clear and the water unusually calm, there being but a slight breeze from the northwest. The mean velocity of the wind, as recorded by the United States Weather Bureau, at the top of the Government building in Erie, was 6 miles per hour, but was probably considerably less than this on the harbor.

The velocity of flow was determined by plunger displacement of the pumps. The Gaskill is a horizontal, compound, fly-wheel pumping engine, erected in 1886, with water cylinders 19½ x 36 ins., and rated at 5 000 000 galls. per 24 hours. The Worthington is a horizontal, compound pumping engine, erected in 1893, with water cylinders 29 x 49¼ ins. (49¼ ins. being the length of the stroke when observations were taken), and rated at 12 000 000 galls. per 24 hours. The Gaskill pump worked against a pressure of 118 lbs., and the Worthington pump against a pressure of 98 lbs., per square inch.

Hook-gauge readings were taken at intervals of one minute in the 24-in. manhole and also in the harbor, about 200 ft. east of this manhole, and Table No. 12 gives the results obtained. One and one-half times the velocity head is deducted from the total loss of head (see Column 10) to allow for loss at entrance and head necessary to produce the velocity, and 5% is allowed for the slip of the pumps (see note at bottom of table). The pumps were overhauled recently, and were in good condition.

The hook-gauge readings taken in the harbor showed the extreme variation of the elevation of the surface of the water to have been ¾ in.

Mr. Fenkell.

TABLE No. 12.—Loss of Head in 8 215 Feet of 60-Inch Cast-Iron Intake Pipe, at Erie, Pa.

September 1st, 1903.

This Intake Pipe was laid in 1895 and 1896.

Time.	Gallons per Minute.						Velocity, in feet per second, from Column 7	Loss of head, in feet.	Loss of head (Column 9) less 1½ times the velocity head.	Loss of head per 1 000 ft., in feet, from Column 10.	Value of c in Chezy formula.	Remarks.
	Gaskill pump.		Worthington pump.		Total pumped.							
	Less 2¼% slip.	Less 5% slip.	See note.	Less 5% slip.	See note.	Less 5% slip.						
(1)	(2)*	(3)*	(4)*	(5)*	(6)*	(7)*	(8)	(9)	(10)	(11)	(12)	(13)
9:00 A. M....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	Closed down pumps.
9:50–10:34...	.....	.....	.....	.....	.0	0	0	0	0	0	.....	
10:35.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	Started Gaskill pump.
10:50–11:20..	1 613.7	1 568.2	.....	.....	1 613.7	1 568.2	0.178	0.02167	0.02092	0.002547	99.8	
11:22.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	Increased speed.
11:25–11:50..	2 569.8	2 497.3	.....	.....	2 569.8	2 497.3	0.283	0.04458	0.04272	0.005200	111.0	
11:52.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	" "
12:00–12:20..	3 467.1	3 369.2	.....	.....	3 467.1	3 369.3	0.382	0.09450	0.09111	0.011091	102.6	
12:30 P. M..	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	Started Worthington pump.
1:00–1:10....	2 620.8	2 546.8	4 029.6	3 905.9	6 650.4	6 452.7	0.732	0.3976	0.38512	0.046880	95.7	
1:11–1:25....	2 475.2	2 405.3	4 489.6	4 351.8	6 964.8	6 757.1	0.767	0.4340	0.42029	0.051163	95.9	
1:27.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	Increased speed.
1:30–1:50....	2 511.6	2 440.1	5 796.0	5 618.1	8 307.6	8 058.2	0.914	0.5507	0.53125	0.064668	101.7	
1:53.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	" "
2:00–2:10....	3 367.0	3 271.9	6 513.6	6 313.7	9 880.6	9 585.6	1.088	0.7736	0.74603	0.090813	102.1	
2:11–2:20....	3 458.0	3 360.3	6 513.6	6 313.7	9 971.6	9 674.0	1.098	0.8108	0.78272	0.095279	100.7	

* Column 6 is the sum of Columns 2 and 4. Column 7 is the sum of Columns 3 and 5. Column 7 is used for computing Columns 8, 10 and 12. Columns 2 and 4 are obtained by multiplying the revolutions by 182 and 552 galls., respectively, as these figures are used at the Erie Water-Works in computing the pumpage. In ordinary practice the length of stroke of the Worthington pump is sometimes increased ½ in.

Mr. Williams. GARDNER S. WILLIAMS, M. Am. Soc. C. E. (by letter).—For a proper appreciation of the value of this paper, as a contribution to hydraulic literature, the following points are to be considered:

1. In number of experiments upon pipes, it exceeds the total of those recorded, up to the time that Darcy began his famous series.

2. In range of velocity and diameter, it exceeds the total of those recorded to that time, with the single exception of fifteen observations by Couplet on a combined lead and earthenware main of about 5 ins. diameter, recorded in 1732.

3. In scope, number and range of experiments, it exceeds any series of investigations on small pipes made, as far as known.

4. It is the first published series of experiments wherein the effect, upon the apparent slope, of variations of the distributions of the internal velocities, was recognized at the time of making the experiments.

5. In detail of preparation and accuracy of execution, it is safe to say that it has not yet been exceeded.

As the importance of the effect upon apparent slope, of irregularities of velocity distribution within the pipe, has been discussed recently, both by the authors and by the writer,* and is certainly now clearly established upon both theoretical and experimental grounds, these experiments are to be regarded as standing upon a higher plane than any of their predecessors, and as marking a distinct step forward in the science of hydraulic experimentation. Deductions fairly drawn from this investigation are therefore entitled to more credence and confidence than those from the far less expertly conducted work of Couplet, Bossut, Du Buat, Venturi, Girard, Gerstner, Young, Rennie, Leslie and Weisbach, upon which the fundamental formulas of flow still in use were primarily based, and even the work of Darcy and Hamilton Smith, Jr., must, in some cases at least, be reinterpreted in the light of more recent developments.

In dealing with a subject involving so many complexities, as does the flow of water, it is very easy to go astray, and conclusions that seemed indubitable when first deduced may be proven in error on further research. Thus the old formulas of flow, of the Du Buat and Prony type, involving two terms in V, one of the square and one of the first power, supposed to represent the effect of the internal resistances and the border friction, respectively, alluring as their hypothesis seems, and supported though they are by the testimony of nearly every experimenter from Du Buat to the present time, appear, in the light of this investigation, to be limited to that brief period of transition from the sub-critical-velocity flow to the ordinary flow, and to have no legitimate existence beyond that range, which is usually considerably below the conditions met in engineering practice. This

* *Transactions*, Am. Soc. C. E., Vol. XLVII, pp. 317, 352 and 356; and *Journal of the Association of Engineering Societies*, March, 1901, p. 177.

deduction becomes the more important and trustworthy when it is understood that at the beginning of their work [the authors accepted the correctness of the old hypothesis, and a chief purpose of the investigation was to determine just how much of the resistance to flow in any given pipe was dependent upon one condition and how much on the other; but, in spite of most faithful effort to locate a law of that nature, as discussed on page 310, the entire evidence of the investigation was found to be against its existence. While it may be possible that, at some future day, facts will be adduced to re-establish the old form of expression, nevertheless, until some such evidence is actually presented, that is at least as fully entitled to acceptance as is that of the authors; on the other side, it seems the part of wisdom to relegate to the historical pigeon-hole all such formulas as Mr. Williams.

$$H_f = a\ V^2 + b\ V + c,$$

and to accept without further demur the form,

$$H_f = m\ V^n,$$

in which the values of n may range from unity to above two, according as the flow is below or above the critical velocity, or the surface is smooth or rough, and the alignment straight or crooked. There is no more reason to insist that H_f vary as V^2 than there is to insist that the power required to keep a train of cars in motion should vary as the square of the speed.

In the mind of the writer, besides the facts developed as to temperature effects, the establishment of the approximate equation,

$$H_f = m\ V^{1.75},$$

for smooth pipes of all diameters, under ordinary flow, a conclusion strongly, almost, it would seem, invincibly, corroborated by the data presented by the authors from the experiments upon large smooth or approximately smooth pipes, from Darcy, Adams, Noble and the Detroit Experiments, entitles this paper to a place of more than ordinary importance. Incidentally, the probability seems good, in view of the result on a very carefully aligned 30-in. Detroit tangent into which the water is known to have entered under some disturbance, and which, nevertheless, gave a value for n of 1.765, that for straight, clean, cast-iron pipe of large diameter, the above formula will be found very closely applicable. This emphasizes the fact that in past experiments it has been necessary to deal with more or less irregular conditions on an assumption that they were regular, and, hence, in attempting to deduce the fundamental formulas of flow, such experiments must be used with great caution, and until the data are at hand from which to produce a formula for the ideal condition of a straight, smooth pipe, it seems questionable to undertake the derivation of one to be rigidly applied to the eccentricities and exigencies of ordinary practice, and the present paper has a decided value in calling attention, as it does, to the wide limits of variation that are to be provided for when such an attempt is made.

Mr. Williams. To those interested in the derivation of the true equation of the distribution-of-velocity curve, the hint that it proves experimentally to be of such form that for the pipe,

$$V m^2 = \frac{\Sigma\ V^2}{\pi\ r^2},$$

will be of value; and when this equation is properly and rationally established, another important step will have been taken toward a true understanding of the laws of flow.

The next investigation needed is one covering with equal care the diameters between the upper limit of the present work and those commonly experimented upon in practice. It is perhaps remarkable that, for diameters from 3 to 12 ins., the only reliable experiments on record are those of Darcy, which, unfortunately, are not strictly applicable to smooth pipes. With the law for straight, smooth pipes once fairly established, both as to m and n, it will be possible to proceed to a study of roughness and curvature effects in a scientific manner. As the laws of flow in a circular pipe are probably less complex than in the case of an open channel, the writer, while fully concurring in the desirability of experiments upon the latter, as suggested by Mr. Thrupp, feels that the pipe work should preferably precede that on open channels, if the two cannot be carried on simultaneously.

The deduction from these experiments, drawn by Mr. Hazen, that the law governing the flow at any velocity is such that the velocity becomes a minimum for the particular slope, is but another exemplification of the Theorem of Least Work, the resistances to flow being the passive forces, and they, therefore, acting according to whichever of the two laws gives them the maximum effect.

Regarding the value of m, it appears to be affected so much more by roughness and alignment than is n that its exact determination may be expected to be difficult, in the general case; and, until experiments are made upon large, smooth, straight pipes, the law of its variation with the diameter must be somewhat in doubt. That such experiments may shortly be made available is most earnestly to be hoped.

Messrs. Saph and Schoder. Augustus V. Saph, Assoc. Am. Soc. C. E., and Ernest W. Schoder, Jun. Am. Soc. C. E. (by letter).—It remains, in conclusion, to comment briefly on some of the points dealt with in the discussions.

Several of the discussions contain references to the two kinds of resistances (page 310). The general question, as far as formulas are concerned, has been investigated quite thoroughly in the paper and the appendix. However, it may be well to point out the general features, which, it seems, a correct analysis must possess, in the light of present knowledge.

Although the motions of the particles of water, in ordinary, or "eddy," flow, may be quite complex, it seems reasonable to conclude

that they obey definite laws. Much more experimental investigation is needed before these can be understood. Nevertheless, there are two established results of hydraulic experiments to which a correct analysis must lead. First, it must account for the "velocity curves," as found by traverses of the cross-sections of the flowing water in pipes and open channels, with the Pitot tube and kindred instruments. Secondly, the result of the integration, for the ideal case of a perfectly smooth pipe, must give a value of n, in the formula, $H = mV^n$, at least as low as 1.75. Messrs. Saph and Schoder.

Mr. Mills' discussion suggests the possibility of arriving at a better understanding of the nature of ordinary flow in pipes by studying the relative effects upon the loss of head of equal changes of temperature in below-critical- and in above-critical-velocity flow. There are on record many useful data for such an investigation.

Mr. Hazen has perhaps misinterpreted the writers' reasoning, as regards the plotting on Plate XI, from which the general formulas were derived. The only line drawn as "a general average" is the full line fitting the writers' experiments on brass pipes. The slope of this is —1.25. All other experiments are grouped into zones, as stated in the paper. The limits of these zones are indicated by broken lines. In spite of the most faithful efforts to average the other experiments, from the standpoints of material, of roughness, or of exponents of V, the writers were forced, finally, to the method of grouping described in the paper.

It seems to the writers that there is no ground for the statement that different values for the exponent of d would be obtained if a comparison were made at a higher velocity than 1 ft. per second. No matter what the unit of measure is, the logarithmic plotting yields the same results, both as regards slope and absolute value of m. If, for instance, the unit of velocity be taken at 3 ft. per second, then a constant, $=3^n$, enters into the equation as a factor of V^n. That the value of the exponent of d can be affected seems unreasonable.

There appears to be no reason why the methods of plotting used by Tutton should give different results from those of the writers. The former used one diagram where the writers used two. The writers treated each series of experiments entirely independently and put the results of these independent plottings on a second general plotting. This independent treatment is practically impossible on a single plotting. Moreover, the writers used as a starting point the line fitting points obtained from some eight hundred experiments on fifteen pipes of almost ideal smoothness, covering a large range of diameters, and made under conditions giving great accuracy. Tutton did not have such an advantage, and the chaos out of which he attempted to make order can only be appreciated by those hydraulic investigators who have attempted the same problem.

Messrs. Saph and Schoder.

Mr. Coker's question, as to the reason for the assumption of a uniform temperature correction of 4% per 10° (page 290), arises from a misunderstanding. The temperature correction applied was not uniform, but varied from 2.9 to 4.6%, as shown in Fig. 3, and as stated in the heading accompanying the figure. The use of the diagram of temperature differences and heads, based upon a uniform 4% correction, was but a facilitating step in the process. The correction for any other percentage was obtained by simple proportion, using the slide rule.

In regard to Mr. Coker's question relating to the protection of the pipes from changes of temperature by means of lagging, it may be stated that there was none. The below-critical-velocity experiments, at low temperatures, on Pipe *XVI*, are the only ones which could have been affected. In relation to this, it may be noted that the writers have been interested in applying the formula (given by Mr. Hazen on page 317) for below-critical-velocity flow to the experiments of Osborne Reynolds and Messrs. Coker and Clement. Reynolds' experiments,* made on lead pipes, 0.242 and 0.498 in. in diameter, with entry lengths of 9.6 ft. in each case, and with temperatures from 41 to 53.6° Fahr., give values of c ranging from 394 to 536. There are twenty-two experiments. Coker and Clements' experiments, of which there are seventy-one, give values ranging from 451 to 527, omitting one erratic value of 771. These latter experiments were made with very careful lagging around the pipe.† The range of temperature, 39.7 to 121.1° Fahr., may be considered as giving a pretty severe test to the Lawrence formula. The extreme values do not seem to have any definite relation to temperature or velocity. The range of values obtained by the writers (as shown in Table No. 10), on five pipes, should be compared with these.

Mr. Fenkell's data on the 60-in. Erie intake pipe are interesting. They were obtained under difficulties which would seem insurmountable to the average experimenter. Nevertheless, considering the conditions, the results are surprisingly good. The writers have plotted logarithmically the values given in Columns 8 and 11 of Table No. 12, the result being

$$H = 0.085\ V^{2.00},$$

in feet per 1 000 ft. Plotting the value of m, 0.085, with the diameter, on Plate XI, it is seen that this pipe may be classed with quite rough or tuberculated pipes.

In concluding, the writers desire to express their high appreciation of the scientific spirit in which the paper has been received and discussed.

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* *Proceedings*, Royal Society, London, Vol. 35, 1883.

† See Footnote, p. 296.

AMERICAN SOCIETY OF CIVIL ENGINEERS
INSTITUTED 1852.

TRANSACTIONS.

Paper No. 965.

THE LEVEE THEORY ON THE MISSISSIPPI RIVER.

An Informal Discussion at the Annual Convention, June 10th, 1903.

SUBJECT FOR DISCUSSION:

"In the Treatment of the Mississippi River, is the Levee Theory Justified by Experience?"

BY MESSRS. B. M. HARROD, L. W. BROWN, J. A. OCKERSON, L. M. HAUPT, B. F. THOMAS, HENRY B. RICHARDSON, T. G. DABNEY AND B. M. HARROD.

Mr. Harrod.

B. M. HARROD, Past-President, Am. Soc. C. E.—"In the Treatment of the Mississippi River, is the Levee Theory justified by Experience?"

The question whether a theory is justified by experience is hardly fair, when its application is quite incomplete, as is the case on the Lower Mississippi River, where the levees have, as yet, neither the grade, section nor extension considered necessary, and the present contents, in cubic yards, are not more than two-thirds of the quantity required by the adopted standard.

The discussion of the subject, however, is opportune, as a recent flood of magnitude has excited interest and afforded much information.

The justification of the "Levee Theory" is involved in such changes in the bed of the river, as a flood channel, as result either

Mr. Harrod. from natural causes or from an increase of the discharge by levees, during more than bank-full stages. If the bed is rising, or the capacity is otherwise reduced by natural and continuing conditions, the completion of a levee system will be prolonged, if not made interminable. If the bed is not rising, and the waterway is maintained or improved, either by deepening or widening, by the discharge of a larger volume at higher velocity, then the problem, though large, is simple and certain.

The Mississippi River Commission, therefore, has given careful investigation to such changes since its appointment. Local and seasonal movements are constantly going on. At certain stages bars build up and pools scour. At others this process is reversed. Besides this, there is a general down-stream and snake-like movement of the sinuosities of the stream. The current binds against the upper and is slack against the lower side of points. Therefore the points, with their opposite concavities, move slowly downward from erosion on the upper, and accretion on the lower, side. The location of the pools and bars has a definite relation to the curvature of the bends, the former lying in the concavities, alternately on the right and left banks, and the bars at the nodes or reversion points between the pools. Hence, as the bends move down stream, the bars and pools move with them. Again, as the result of caving on one and accretion on the opposite bank, the river shifts sideways. Instances are not wanting where this movement has amounted to its entire width in fifteen or twenty years.

It is evident that with these unstable conditions but little can be learned from isolated or scattered soundings. A cross-section line over a bar may, in a few years, lie through a pool, or the river may have slipped to one side, leaving it on dry ground.

In the years 1881, 1882 and 1883, the Commission made an exact and detailed survey of the river from Cairo to the head of the Passes, a distance of 1 063 miles, with cross-sections averaging about four to the mile, and seventy-five soundings to the line. There was no better way of investigating this difficult and important question than by repeating the survey. This was done in 1894, 1895 and 1896, after an interval of thirteen years, over that part of the river where the levee system had been most improved during the interval, from the mouth of White River to Donaldsonville, La., a distance of 472 miles. This second survey was made in greater detail, in order that it might better serve for future comparisons.

There is a limit to the value of the results obtainable even by this exhaustive process, of which the Commission was aware, but no better method seemed available. A comparison between the two surveys would be conclusive in proportion to the similarity of the stage conditions preceding them and prevailing while the parties were in the field. Each

survey was of such magnitude and detail as to require several months, and it was improbable that there would be a close repetition of the conditions of the first during the second. Mr. Harrod.

Both surveys and the analysis of their elements were made under the charge of J. A. Ockerson, M. Am. Soc. C. E., and his detailed report on them is found in the Reports of the Mississippi River Commission for 1896 and 1897, published, respectively, in the sixth and fifth parts of the Reports of the Chief of Engineers for those years.

The following conclusions from his study of the conditions are well founded:

The differences found in these two surveys do not necessarily represent all the changes or the resultant of all the changes that may have taken place between them. During this time the conditions of the river bed may have varied in both directions from those found in either survey. They should, however, indicate any trend, or persistent and progressive change that has taken place. This general tendency seems to be toward a channel more uniform in depth and of greater capacity.

Table No. 1 gives the results from the Arkansas River to Vicksburg, 200 miles.

TABLE No. 1.—Relation of Elements in 1894 to those in 1881–82.

	Low water.	Medium stage.	Bank-full.
Width	+3.6 per cent.	+8.3 per cent.	−0.08 per cent.
Mean depth	−6 "	−6 "	+4.5 "
Area	−3.6 "	+2.7 "	+3.1 "
Hydraulic radius			+8.25 "

A composite comparative diagram of the sections in this part of the survey is shown in Fig. 1.

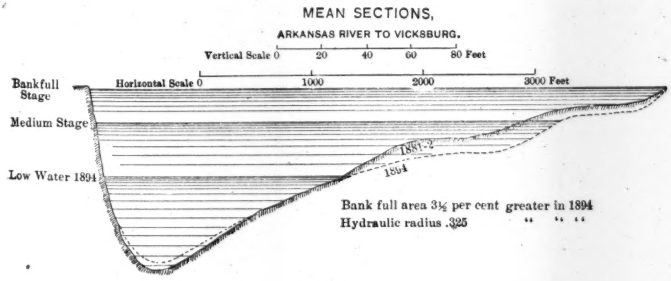
MEAN SECTIONS,
ARKANSAS RIVER TO VICKSBURG.
Vertical Scale 0 20 40 60 80 Feet
Horizontal Scale 0 1000 2000 3000 Feet
Bankfull Stage
Medium Stage
Low Water 1894
1881-2
1894
Bank full area 3½ per cent greater in 1894
Hydraulic radius .325 " " "

FIG. 1.

Mr. Harrod. The mean of the results from Vicksburg to Donaldsonville, La., 272 miles, is given in Table No. 2.

TABLE No. 2.—Relation of Elements in Survey of 1895–96 to those in 1882–83.

	Low water.	Medium stage.	Bank-full.
Width	+3.6 per cent.	+3.8 per cent.	+7.5 per cent.
Mean depth	−5.9 "	−8.1 "	−8.9 "
Area	−3.8 "	−0.8 "	+1.4 "

The conditions under which the two surveys from Vicksburg to Donaldsonville were made were so different as to give abnormal results. The first (1882) was after the greatest recorded flood and on the rise of the succeeding one, which was of considerable magnitude, while the last (1895–6) was preceded by two seasons of extreme low water, and a very moderate intermediate flood, during which sediment transported from above would be deposited in the lower part of the river, particularly below Red River, where a relatively large low-water section, flat slopes, and correspondingly small velocities are found.

A duplication of the survey of 1881 is now being made over that part of the river from Cairo to the mouth of the Arkansas, along which the levee system has been much extended since 1895. In the future, at proper intervals of time, similar resurveys will be made over the entire river, until they yield indications of a persistent and progressive change.

It may be assumed that the low-water plane conforms to the shape of the river bed, and that any elevation or depression of the latter, as the result either of natural causes or of levee building, will be recorded in the low-water gauge readings. The improvement of levees during the past twenty years, and their effect in increasing the height of floods, has been most marked in the 500 miles of river in which are included the gauge stations of Fulton, Memphis, Greenville and Lake Providence. There has, as yet, been no levee building which has affected the flood stage at Cairo. The effect on the bed of the river, therefore, may be observed by comparing the low-water stages at the points where levee improvement has been greatest with those at Cairo where no influence of the kind has been felt.

Prior to 1882 the United States had built no levees, and the insufficient and incomplete State levees existing at the time were badly wrecked by the flood of that year, which left them in quite an unserviceable condition.

If the average of the low waters at the points mentioned above, which have been selected as fairly representative, for the five years

following this disaster of 1882 (1883–84–85–86–87) and that of the last five years (1898–99–1900–01–02) be compared with the averages of the low waters at Cairo during the same two periods, as a standard, there will be observed, during the latter period an average relative depression of the low-water surface of 0.74 ft. at Fulton, 0.68 ft. at Memphis, 1.60 ft. at Greenville, and 1.89 ft. at Lake Providence. Mr. Harrod.

These reductions of the low-water plane are indicative of a depression of the bed, and are proportionate to the duration and degree of levee maintenance and improvement in the vicinity of the gauge stations mentioned.

Table No. 3 will make this statement clearer.

TABLE No. 3.

Average low water.	Cairo.	Fulton.	Memphis.	Greenville.	Lake Providence.
1883-87	5.21	4.50	2.57	5.56	4.04
1898-1902	5.35	3.90	2.03	4.10	2.29
Difference in averages	+0.14	−0.60	−0.54	−1.46	−1.75
Reduction below Cairo, 1898-1902		0.74	0.68	1.60	1.89

It was observed, in the great flood of 1897, that:

"The first gauge below Red River to exceed its previous record was the lowest one on the river, at Fort Jackson. The next was the Carrollton gauge, and so on, up to Red River, where the gauge did not exceed its previous record until sixteen days after the Carrollton gauge had done so. When the Carrollton gauge had reached its previous maximum, that at Red River still lacked 1.6 ft. of the height which had produced that maximum."

The same prematurity of rise at the lower gauge stations occurred during the present year. In a discussion of this phenomenon by Major Derby, M. Am. Soc. C. E., in the Report of the Mississippi River Commission for 1900, it was considered as due to one of only three causes: (1st) a raising of the bed of the river below Carrollton; (2d) the effect of crevasses and their closure, or (3d) an increase of the carrying capacity of the channel between Red River and Carrollton, by which the resistance to discharge and the slope over that 200 miles of river was reduced. His analysis of flood waves ranging in height at Red River from 19.3 to 45.2 ft. between the years 1872 and 1899, discredited the first two causes, and led to the conclusion that the discharge capacity of this part of the river had been increased during the period under consideration.

Besides these extended comparative observations, others of a more local character have been made, in connection with crevasses or

Mr. Harrod. temporary outlets, as at Malone's, Riverton, Bolivar, Mound Place, Morganza, Bonnet Carré and Cubitt's Gap. Whenever the resurvey was made after the occurrence of an outlet, it showed a reduction of the cross-sectional area below. When made after closure, an enlargement has been observed.

When, in 1880, the river was first subjected to continuous observation, the levee system was in its infancy, some basins were entirely unleveed, and such crude levees as existed were breached at many places by every high water. It was then noticed that the rise and fall was very different at different places. When classified, the greater annual oscillations, amounting generally to about 45 ft., were found at or near the mouths of the tributaries, the Ohio, St. Francis, Arkansas, Yazoo and Red Rivers, while the lesser ones, averaging only about 35 ft., were observed at intermediate points along the fronts of the great basins drained by these tributaries, as at Fulton, Memphis, Greenville, Lake Providence and St. Joseph.

The gauge readings, when plotted, showed a smooth and regular high-water slope, while that of the low-water slope was quite irregular, being depressed about the junctions of the tributaries, and raised between them, or along the basin fronts. A diagram of the high water of 1882 and the low water of 1883, Fig. 2, shows that these differences in annual oscillation were caused, not by the rise, but by the excess of fall at the tributaries over that on the bars of the elevated bed of the river between them.

It was further indicated by the discharge observations taken at high waters, at the places near the tributaries, and at the others along the basin fronts, that the discharges at the former were about 1 500 000 cu. ft. per second, and exceeded those at the latter, or intermediate points along the basin fronts, by several hundred thousand feet.

This difference, from a quarter to a half million feet at times, had escaped from the river bed, over the banks, into the basins, and was returned to the main river below through the tributaries which are the outfalls for their normal and overflow drainage. Where the river discharged between banks the entire flood volume, the bed was deepened; and where it discharged only two-thirds of that volume, the bed was shallowed. The depletion of a thousand floods by overflow had impressed this shape upon the bed.

A part of the "Levee Theory" is that the escape of flood water from the river along the fronts into the adjacent basins caused the elevation of bed that existed, as evidenced by the low-water slope; and that, when this is prevented by levees, and the discharge confined, a primary effect will be the reproduction, in the high-water slope, of those elevations which have been observed and described in the low. This has already been brought about by the extension and improvement of levees, and is measured by the excessive height of recent

Mr. Harrod.

HIGH-WATER SLOPE OF 1882, AND LOW-WATER SLOPE OF 1883.

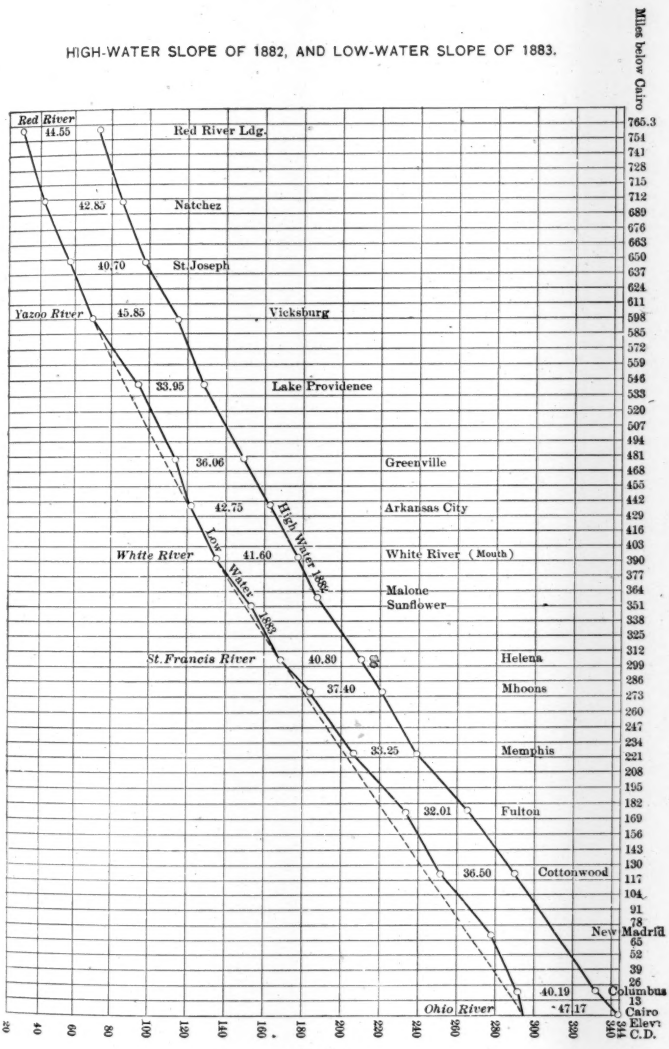

FIG. 2.

Mr. Harrod. floods at points situated along the middle of basin fronts, as Memphis, or Lake Providence. It will be observed that an equal increase of heights has not occurred at the mouths of the tributaries.

Another part of the "Levee Theory" is that a reversal, or removal, of the conditions which have contracted one part of the waterway and relatively enlarged another, of the same river, will remove these differentiations, and that with a uniform discharge for each stage, from Cairo to the sea, affected only by increments from the normal drainage of the basins, through an erosible bed which the river has moulded to its needs, these irregularities of slope, velocity and section will disappear, and that there will result a regular and substantially parallel slope curve, flattened a little by each increment of volume from a tributary, until Red River is reached, and from thence down the slopes at all stages will converge to sea level.

If the flow is as great along the basin fronts as at the tributaries, why should not the channel capacity of this strictly sedimentary stream be as great at one place as at another?

The condition in which the Commission found the river, and of which a description has been attempted, is the result of many centuries of alternation of channel depletion and enlargement at every flood stage. It is not to be expected that an accumulation of deposit, almost geological in its age and its mass, will be removed by five or ten years of levee improvement, or by a few great floods occurring at intervals of five or six years. But, with the force and time we have on hand, the result is not in doubt. The evidence of a start in this direction is given in a previous part of this discussion.

The result of all observations seems to show a general tendency to an enlargement of the stream, that its capacity for flood discharge has been more than maintained, and that the apprehension of its deterioration, from natural causes, or from levee building, may be dismissed as, at least, unproved, if not disproved.

Consideration must also be given to the floods of the future, which will seek an outfall through the channel below Cairo. Will they be of greater volume than those of the past? They come from four sources, the Upper Mississippi, the Ohio and the Missouri water-sheds, and from the tributaries of the main trunk below Cairo. It may generally be stated that, when the first three form a combination which causes a dangerous stage on the Lower Mississippi, about two-thirds of the discharge, or 1 000 000 cu. ft. per second, is contributed by the Ohio. This is the controlling factor in great floods.

While the relation between deforesting and precipitation is assumed rather than established, there is no doubt that the processes of clearing, draining and cultivating may materially affect the distribution of the run-off, delivering to the streams of outfall a larger share in a shorter time, and tending to higher high waters and lower low waters.

Under certain topographic conditions, these results may be limited and even reversed. It is, therefore, an important part of this discussion to consider the bearing which the conditions of the four sources of supply have had, and will have, upon the high waters of the Lower Mississippi. Mr. Harrod.

It seems probable that the future changes in the flood conditions in the Upper Mississippi Basin will be slight. The forests, or those having commercial value, have been very largely cut down, cultivation under improved methods is already greatly extended, the reservoir system may be increased, and while the projected discharge of the Chicago Canal, constituting about one-tenth of the low-water discharge of the river below Cairo may be appreciably beneficial to low-water navigation, its contribution of about one-half of 1% to the flood volume is too small for consideration.

It is unfortunate that the records do not extend far enough back to give a life history of these tributary rivers, the gauges having generally been established within the last thirty years. Fortunately, since it bears on the most important flood factor, Cincinnati is an exception, having a continuous record of forty-five years. An examination of this shows that, if this period is divided in halves, the average of flood heights on the Ohio in the first half is 48.80, and in the second 52.37 ft. If, however, it is divided into thirds, they give the following relation of averages for the three periods: 48.51, 52.57, and 50.69 ft. For low water, the average result for half periods is 3.80 and 3.86 ft., and for thirds is 3.83, 3.60 and 4.06 ft. It does not appear, therefore, that on the Ohio River, for the last forty-five years, there has been a progressive change to higher high waters and lower low waters (although the processes to which such a result is usually attributed have presumably continued), but rather that some conservative or restorative influence has been in operation.

The high and low waters at Cincinnati for the period under consideration are shown in Fig. 3.

The physical conditions in the Basin of the Missouri are materially different from those in the Ohio. Except about the headwaters, it is a region of gentler slopes, largely without forests. Its progressive occupation and cultivation will be accompanied by plowing and planting surfaces which are now smooth and barren, and probably by a great development of reservoir building for irrigation. The tendency of these processes should be to check the rapidity with which its floods are discharged, and render less likely their coincidence with those of the Ohio, which generally culminate in February or March.

The tributaries below Cairo may be grouped together for consideration. While some of them head in arid regions similar to those drained by the Missouri, they generally flow through flat alluvial lands, where drainage is and always will be slow, and where the prevailing

Mr. Harrod.

forests will be gradually replaced by a cultivation which will not materially hasten the run-off. With overflow excluded from the basins, there is no reason apparent why the natural discharge of these drainage systems should be materially increased in the future.

If not levees, what then? Reservoirs, or Outlets?

The tendency of any extension of a reservoir system on the Upper Mississippi or Missouri would be to abate floods on the Lower Mississippi, but probably to a degree hardly appreciable. Such a system on the Ohio, if practicable, might produce more important results. But the late Milnor Roberts, Past-President, Am. Soc. C. E., closed this part of the subject forever in his most able report of 1870.

Outlets have received theoretic support in the report of Humphreys and Abbot, and of the United States Commission of Engineers of 1875 for the reclamation of the alluvial basin from overflow, but, after a detailed examination, they are unanimously:

"Forced unwillingly to the conclusion, that no assistance in reclaiming the alluvial region from

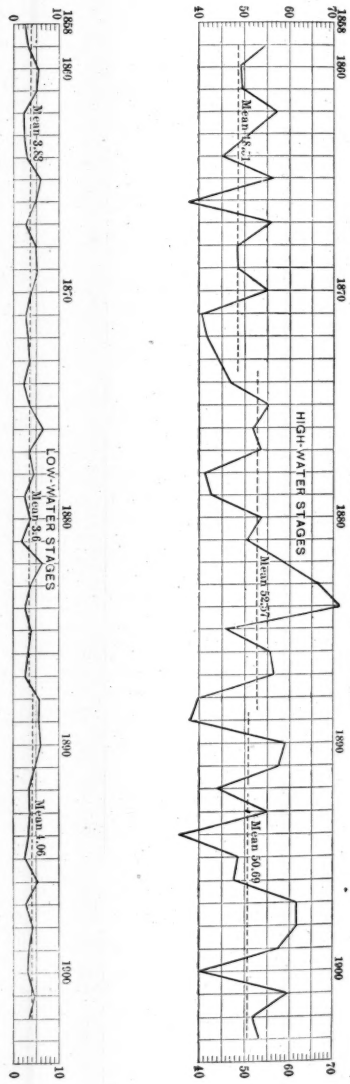

FIG. 3.

overflow can judiciously be anticipated from artificial outlets. They are correct in theory, but no advantageous sites for their construction exist." Mr. Harrod.

The views on which these theoretical conclusions in favor of outlets were based, *viz.*, that the bed of the stream was in a material so inerosible, and that changes in volume and velocity bore so little relation to scour or deposit, that its shape and dimensions would not respond to these changes, have not been upheld by more recent and exhaustive observations. These show that the bed is in a material which is being moved by the current from day to day and from bar to bar, and that its shape and dimensions are the resultants of this force.

The experience in levee building is that the limit to which the use of the material and methods of their construction can be safely extended has not yet been approached. The larger levees, which reach, in sloughs or other depressions, a height of 30 to 40 ft., and even more, are generally considered as among the safest.

Many substitutes and reinforcements for the earthen embankment, of various materials and construction, wood, stone, steel, concrete, etc., have been proposed. But when all things are considered, including ease of construction, economy and endurance, the outlook at present is that a carefully constructed earthen levee system, with sufficient grade and section, when properly cared for, presents advantages with which competition will always be extremely difficult.

There are two natural conditions prevailing on the Mississippi below Cairo which add materially to the practicability and efficiency of the "Levee System." One is the general presence of Bermuda grass, which grows with closely interwoven stems, attached to the ground by root tufts at close intervals, and forms a dense sod, presenting great protection against wave wash. The other is the high charge of sediment carried by the river at its higher stages. As the water seeps into and through the levees, it is filtered, the particles being deposited in the interstices of the soil of which it is built. The clear water percolates through to the land slope, while its charge of sediment remains and gradually diminishes the leakage.

The experience concerning the cost of making good the losses sustained by the existing levees, from caving banks and breaks from other causes, has been collated for the past eight years. It amounts annually, during that period, to a little less than 1½ per cent. The losses by the flood of this season and by certain important works of renewal now in sight will probably temporarily increase this annual cost in the near future. Also, as the levees approach the grade and section which it is considered necessary to give them, and their contents per linear foot are thus increased, the work of closing any gap that may occur will be proportionately greater. On the other hand, the better locations, construction and care which are already made possible, to

Mr. Harrod. a certain degree, and which can, in the future, be practiced to a still greater extent, from the more liberal and regular supply of funds, should tend to a reduction of the annual losses from caving banks, and the occasional losses from extreme high waters.

This increase of resources can be expected both from a fuller realization by the General Government of the importance of the work which it has undertaken, the reclamation from overflow, and the agricultural and commercial development of 20 000 000 acres of the most fertile soil, and from the increasing number and wealth of the communities now occupying and improving these lands.

The amount applied to the extension and improvement of the levee system of the Lower Mississippi in the year 1900 was about $2 961 000. Of this, about $1 000 000 was allotted from the appropriation by Congress for the river below Cairo, and the remainder was supplied by the levee organizations of the riparian States. This is, substantially, the division of cost which has prevailed since the Government has shared in the construction of a levee system. Several of these local organizations now feel justified, by their experience with the "Levee Theory" to seek legislative authority for an increase of their contribution by additional taxation and bond issues.

When the levee system of the Lower Mississippi shall have been completed, it will still be but an engineering structure, subject to the vicissitudes of time and accident. It will need constant care, and occasional renewal of parts. Crevasses will occur as long as trains are derailed, or collide, as ships are wrecked, or fire-proof buildings are destroyed. A crevasse in the levee of the future will be a more serious disaster than in one of the present time, in proportion to its greater depth and discharge and the greater improvements which have developed under its protection. But this is the case with all of our work, whose progress has not been deterred by the greater risks which are necessarily assumed in meeting the demands of modern civilization.

This discussion, already too long, will be closed with a short comparison of the floods of 1897 and 1903 and their results.

The computations of the discharge measurements of the last flood are not yet complete, but it is apparent that the maximum discharge of 1897 was slightly more than that of 1903. Greater heights were generally reached this year, mainly along the fronts of the basins where levees have made the greatest reduction in the overflow, but not at the mouths of the tributaries. This increased height was due both to the extension of levees along hitherto unleveed fronts, and to the improvement made in the existing lines since 1897, which enabled them to exert more resistance and control.

The greatest increase of flood height this year was about 3 ft. at Memphis, where levees have been but recently extended. Nevertheless, there were, in 1897, between Cairo and New Orleans, a distance of

960 miles, forty-three crevasses, while during the past flood, so great had been the improvement of levees in the meantime, there were but six. While the limit of the overflowed area has not yet been completely ascertained, it is known to be reduced largely, if not quite in proportion to the lesser number of breaks. Mr. Harrod.

Table No. 4 gives certain high-water records. In the third column is given a standard, adopted provisionally as the heights which great floods might be expected to reach when controlled by levees. The other columns are explained sufficiently by their headings.

The experience of 1903 makes advisable a revision of the provisional standard in the vicinity of some of the gauge stations.

TABLE No. 4.—HIGH-WATER RECORDS.

Gauges.	Miles below Cairo.	Standard high-water.	Highest before 1903.	Highest in 1903.	1903 compared with previous highest.	1903 compared with standard.
	Miles.	Feet.	Feet.	Feet.	Feet.	Feet.
Cairo	.....	54.17	52.17	50.60	—1.57	—3.57
Columbus	21.3	48.10	45.58	44.40	—1.18	—3.70
New Madrid	70.3	42.90	41.50	39.50	—2.00	—3.40
Cottonwood Point	122.5	42.20	39.35	40.00	+0.65	—2.20
Fulton	175.4	40.40	38.30	40.10	+1.80	—0.30
Memphis	230.0	41.60	37.66	40.60	+3.00	—1.00
Mhoon	276.3	46.20	41.60	41.80	+0.20	—4.40
Helena	306.5	54.10	51.75	51.00	—0.75	—3.10
Sunflower	352.7	50.20	47.17	48.00	+0.83	—2.20
White River	393.2	56.40	52.42	53.70	+1.28	—2.70
Arkansas City	438.3	56.30	51.90	53.00	+1.10	—3.30
Greenville	478.3	50.50	46.75	49.10	+2.35	—1.40
Lake Providence	542.3	48.00	44.54	46.40	+1.86	—1.60
Vicksburg	599.3	55.00	52.48	51.80	—0.68	—3.20
St. Joseph	648.3	50.80	47.85	48.00	+0.15	—2.80
Natchez	700.3	54.00	49.82	50.40	+0.58	—3.60
Red River Landing	765.3	52.50	50.20	50.00	—0.20	—2.50
Bayou Sara	799.8	45.70	43.70	43.45	—0.30	—2.30
Baton Rouge	833.3	43.20	40.65	40.00	—0.65	—3.20
Plaquemine	854.1	38.70	36.25	36.10	—0.15	—2.60
Donaldsonville	885.4	34.95	32.75	32.20	—0.55	—2.75
College Point	904.5	29.80	27.95	27.80	—0.15	—2.00
Carrollton	957.0	20.35	19.17	19.40	+0.23	—0.95
Fort Jackson	1 039.0	8.00	7.20	8.00	+0.80	—0.00

It will be observed that no high water has yet reached the predicted standard.

The engineers engaged in the reclamation of the Valley of the Mississippi River from overflow know more about levee building than they have yet had the opportunity of putting in practice. They are quite aware of many and much-needed improvements, both in their construction and in their care and preservation. Up to the present time, the compelling need has been, and for several years will be, continuity of line, higher grades and standard sections. Those used provisionally are everywhere below those considered safe for great floods, and the

Mr. Harrod. present contents of levees are not more than 60% of what is considered necessary for satisfactory protection.

Yet, behind this partial shelter, population has increased, values have risen, wealth has accumulated, comfort and culture have developed, and great railroad systems have extended, at such a rate that it can be said that the reclamation of this region is one of the most successful and beneficent public works now in progress.

Mr. Brown. L. W. BROWN, M. Am. Soc. C. E. (by letter).—A quotation from Bayard Taylor's ode to the Nile applies very appropriately to the Mississippi River:

"Mysterious Flood,—that through the silent sands
Hast wandered, century on century,
Watering the length of great Egyptian lands,
Which were not, but for thee."

The Mississippi River, from its mouth to the head waters of the Missouri, has a length of 4 190 miles, and is, perhaps, the longest river in the world. The navigable waters of this great river and tributaries are estimated as having an aggregate length of more than 15 000 miles.

The river drains an area embracing 56° of longitude, and 21° of latitude, aggregating 1 240 050 sq. miles; or 41% of the whole area of the United States, exclusive of Alaska.

Were it not for this great river, the main trunk of which flows through the very core of our country, we would not have the vast area of rich alluvial lands which constitute, not only a large portion of our country, but a most productive and valuable portion; and we would not have, as a subject for discussion, the protection of this valuable territory from the ravages of inundation.

Referring to the observations of Taylor on the Nile, the first thought is that this large and valuable section of our country was made by the Mississippi River according to the well-known and established laws of Nature; and the further thought is suggested: Would civilization, had it existed during the period when this system of making a country was inaugurated by Nature, have allowed it to be done?

If an answer were to be made in accordance with the methods adopted by man in all his works, thus far, in connection with the Mississippi, it would, of necessity, be in the negative, and, as a consequence, we would to-day have no valuable and extensive territory to protect.

It is perhaps a most wise provision of Nature, that her large and extensive undertaking, in making a rich and productive country, was inaugurated, and nearly completed, before civilization was introduced, otherwise we might now be compelled to live and have our habitations in floating craft, in order that all the available land could be utilized for agricultural purposes to support us until we could complete the filling of the deep seas and oceans, which we are now straining every energy to accomplish.

When man first settled in this rich alluvial territory, about 185 years ago, he immediately began the construction of levees, and thus was inaugurated the principle which has been extended and enlarged until the whole length of the river is now provided with a range of mountains on each side, almost completely confining the waters; and the old-fashioned method of building a country, adopted by Dame Nature centuries ago, has been entirely discontinued; and this method of Nature's, like the fashion of dress worn by our forefathers, is, in this enlightened age, considered out of date. Mr. Brown.

That levees are necessary for the proper protection of the alluvial territory from inundation from the river admits of no argument. They are absolutely necessary; but, the protection of the territory, by confining the entire volume in the main trunk, in order to improve navigation, is most apparently disastrous to both protection and navigation, and it is reasonable to assume that we must either abandon our present methods of protection, in attempting to confine all the flood volume in one main trunk, or be satisfied to lose eventually, not only the money and energy expended in the work, but also all the benefits of our large and valuable alluvial territory, as Nature will not long submit to being "straight-laced and corseted."

Voltaire says: "The progress of rivers to the ocean is not so rapid as that of man to err."

It is well within the line of possibility, considering the past and existing conditions as regards the Mississippi floods, that man has erred, and erred sadly, in attempting to interfere with Nature's laws, as he has no doubt done, in an endeavor to confine the whole flood volume in one main trunk, and not allow the low places to be elevated and the swamps reclaimed for the support, health and prosperity of future generations.

It would appear to be unnecessary, in the light of the information now available, and of the facts now demonstrated, to detail the many reasons why the total confining of the waters of the Mississippi would not be disastrous to all interests, not only for the present, but for all future generations.

The very fact that we are neglecting an opportunity to build up the low places, reclaim swamp areas, and thereby improve the health of the territory, is sufficient evidence that we are not making a proper use of Nature's gifts; and it is possible that our efforts to confine the floods are a violation of Nature's laws.

The futility of attempting to confine the flood waters of the Mississippi is demonstrated most thoroughly and convincingly by a consideration of the work executed during the past twenty-five years, and the results secured.

Prior to the appointment of the Mississippi River Commission by the Federal Government, in 1878—twenty-five years ago—no general

Mr. Brown. or systemized plan for leveeing the river was attempted. Each State constructed levees, for protection only, on such lines and such locations as best served their several interests. At this time, or prior, there was no thought of confining the whole volume of floods.

Since 1878, a combined effort, requiring the expenditure of a very large amount of money and energy, has been made by the United States Government, the States and Parishes, and by the individual interests bordering on the stream, to make a continuous levee system throughout the whole length of the river, and to retain entirely the flood waters within the trunk of the river, and not allow the low places and the swamps, which comprise a very large area, to assist in lowering the flood plane.

This system of levee construction was inaugurated 185 years ago, but only during the past few years has it reached such a stage of completion that its effects could be observed impartially and practically, and determined with reasonable precision; and, while the system is still far from completion, we have now a clear and positive conception of the utter futility of securing from the further completion of the system any reasonable or satisfactory results.

The ultimate disastrous results of the attempt to confine the flood waters of the Mississippi (aside from the tremendous losses from inundation, which are known and apparent to all) are most eloquently portrayed by a close observation of the flood condition during the time when but a very small amount of the work, forming the principle of the present system, was completed, as compared with the past few years when the system has been extended very considerably.

Comparing the flood heights at Carrollton, La., we find that the great flood of 1828 reached an elevation of 14.85 ft. above sea level. The great flood of 1851 reached an elevation of 15.05 ft. above sea level, or 0.2 ft. higher than the flood of 1828. The great flood of 1874 reached an elevation of 15.35 ft. above sea level, or 0.3 ft. above the flood of 1851, or 0.5 ft. above the flood of 1828. The great flood of 1897 reached an elevation of 18.72 ft. above sea level, or 3.37 ft. higher than the flood of 1874, or 3.67 ft. higher than that of 1851, or 3.87 ft. higher than that of 1828. The flood of 1903 reached an elevation of 19.05 ft. above sea level, or 0.33 ft. higher than the flood of 1897, 3.70 ft. higher than the flood of 1874, 4.0 ft. higher than the flood of 1851, and 4.2 ft. higher than the flood of 1828.

It is instructive to observe:

1st.—That during the twenty-three years, from 1828 to 1851, the increase of flood plane was only 0.2 ft., and that during this period of twenty-three years very little protection work was executed.

2d.—That during the twenty-three years from 1851 to 1874 the increase of flood plane was only 0.3 ft., and that during this period of twenty-three years considerable protection work was executed, but it

was small as compared with that of the following period of twenty-three years. Mr. Brown.

3d.—That during the twenty-three years from 1874 to 1897 the increase of flood plane was 3.37 ft. It was during this period of twenty-three years, beginning in 1878, that great activity was displayed in the construction of levees, and in 1897 these levees confined a very considerable portion of the river.

4th.—That in the short period of six years—1897 to 1903—the increase of flood plane was 0.33 ft. During this period of six years the levee system was considerably enlarged, strengthened and extended, but was far from being completed.

5th.—That in 1828 there was practically no protection work.

6th.—That in 1851 and 1874 the protection works were very limited, both as to extent and dimensions.

7th.—That in 1897 and 1903 the protection works were very large, both in extent and dimensions.

8th.—That in none of these years of high floods have the protection works been maintained against failure.

9th.—That we have no method of determining positively what would have been the elevation of the floods, or other conditions, had the protection works not failed.

10th.—That there is no known or proven formula, guide, rule, or precedent by which works of protection, which embrace the confining of the flood waters, can be calculated, designed or constructed with any degree of safety.

11th.—That we have no absolute or positive evidence that the meteorological and climatic conditions during these years of high floods were abnormal, or that these conditions, if abnormal, during these years, will not be surpassed in their intensity to precipitate floods in the future.*

12th.—That the elevation of normal high water in later years, or from 1897, has reached a higher elevation above sea level than did the great flood of 1874 or of those previous to 1874.

The foregoing cursory examination of flood conditions, covering a period of seventy-five years, provides convincing arguments which show most conclusively that we may not, with entire economy or safety, confine the flood waters of the Mississippi; and there exist other arguments and reasons, equally convincing and tenable, which will be referred to briefly as follows:

A.—The present status of the material forming the alluvial basin through which the river runs, is such that even with low velocity, a uniform, constant and proper waterway or channel cannot be maintained, as is possible in older and more stable territory; and, as a con-

* The extraordinary floods in Kansas, Nebraska and Iowa, during the past few days, or since this discussion was prepared, demonstrate most emphatically the correctness of the views herewith presented, especially in reference to future floods.

Mr. Brown. sequence, there would be no assurance, even if the volume of a flood could be calculated and known, that the trunk would receive and discharge it safely and properly, as the discharging capacity of the river at a given elevation of flood plane, is, and will so remain for centuries to come, an absolutely unknown quantity—never the same for two seasons in succession—due to the constant changing of bed, and other conditions. Hence the successful confining of the floods within the trunk is scarcely within the line of a possibility.

B.—The high velocity required to discharge the large flood volumes, if confined in the main trunk of a river which flows through a new formation such as that comprising the Mississippi alluvial, would be such as to abrade constantly the high banks which Nature built originally, and carry them to the sea, and thus not only destroy what Nature accomplished for the benefit of man, but require the reconstruction of levees, upon lower, newer and less stable ground, which would not only increase the cost largely, but increase very largely the danger of their failure.

C.—The confining of the floods of the Mississippi will, in time, result in the decrease of the cross-section now existing below the natural bank, and, with the same volume of discharge, will raise the elevation of the flood plane. This statement is made with the writer's full and positive belief that the increase of velocity necessary to discharge the confined flood volumes of a stream flowing in alluvial territory, and which tears down banks and builds shoals in a heretofore deep pool or reach, must, of necessity, gradually but surely shoal the bottom and foul the sides, thus decreasing the original cross-section. This statement and belief is substantiated by the history of alluvial rivers in older countries, and is also verified by an examination of the cross-section of the river at Carrollton for a period covering seventy years.

Table No. 5 is constructed to show that in seventy years the cross-section below the natural bank at Carrollton has decreased.

TABLE No. 5.

Flood year.	Elevation above sea level, in feet.	Velocity, in feet per second.	Discharge, in cubic feet per second.	Cross-section, in square feet.	Height above flood of 1828, in feet.	Area above flood plane of 1828, in square feet.	Area below flood plane of 1828, in square feet.	Area fouled since 1828, in square feet.
1828	14.85	5.90	1 099 000	186 271	0	0	186 271	0
1851	15.05	5.99	1 118 000	186 644	0.2	580	186 064	207
1874	15.35	6.75	1 175 000	174 074	0.5	1 450	172 624	13 647
1897	18.72	7.30	1 350 000	184 800	3.87	11 223	173 577	12 694
1898	15.30	6.28	1 084 000	172 611	0.45	1 305	171 306	14 965

Mr. Brown.

The discharge data for 1903, when it is received, will no doubt furnish very interesting additional information.

It will be observed that the cross-section below the flood plane of 1828 was 207 sq. ft. less than in 1851; the section in 1874 was 13 647 sq. ft. less than in 1828; in 1897 it was 12 694 sq. ft. less; and in 1898—normal flood—14 965 sq. ft. less.

The discharge data for 1896, if obtainable, would be very interesting to show what scour was occasioned by the high velocities of 1897—which velocities, as recorded by scientific investigation, were sufficient to abrade stratified rock—and it is somewhat surprising that the high velocities of the flood of 1897 did not recover the section as it existed in 1828. It will also be noted that the section below the flood plane of 1828 was, in 1898—a year of normal flood—2 271 sq. ft. less than in 1897.

The application of Kutter's formula for the discharge of streams shows that the floods of the Mississippi are not consistent under comparative conditions. The flood of 1851, which had a cross-section of 186 644 sq. ft. and a slope of 0.125 ft. per mile—by formula—would have a velocity of 6.22 ft. and a discharge of 1 160 926 cu. ft. The actual gauging of the stream was 5.99 ft. velocity and 1 118 000 cu. ft. discharge. The flood of 1897, when the area was 184 800 sq. ft. and the slope 0.156 ft. per mile—by formula—would have a velocity of 6.512 ft. and a discharge of 1 203 417 cu. ft. The actual gauging, however, shows 7.30 ft. velocity and 1 350 000 cu. ft. discharge.

D.—Our country, to-day, is rich, prosperous and powerful, and can, perchance, by great exertion, raise the necessary funds to construct and maintain these very large and expensive levee lines to confine the river (but it can never be accomplished and absolute safety assured); but in times of adversity, which every country may expect to experience, and funds are lacking to maintain them properly, what will become of this great levee system, which requires constant watching, guarding, and the annual expenditure of large sums of money? And if the levee system, when constructed, if it were possible, to the acme of success, as far as confining the flood waters is concerned, is by necessity forced to be neglected, what becomes of our rich alluvial country?

Works affecting such enormous values, as do the works of protection against inundation by the floods of the Mississippi, must, of necessity, to be worthy of adoption, be such as will not confiscate the country in their construction; and also such as will serve reasonably well the purpose for which they were constructed, were they, from expediency, forced to be neglected for a time.

E.—In time of war, the levee system of the Mississippi, if it could be constructed to confine ultimately the flood volumes, would be of national and international importance as an engine of warfare, and its

Mr. Brown. control would be contested hotly. To the country owning it, it would present an ever-abiding menace, for should an enemy obtain control, for a few hours only, of any part of the territory embracing the system, a very powerful and readily-operated weapon would be secured, as with great dispatch, effectiveness, and at the least possible cost, a whole country could be submerged and devastated, and the operations of a whole army be practically stopped.

F.—The continuation of the present system, and the attempt to confine the flood waters of the river, will require the construction, in the very near future, of levees of enormous proportions, which will be a very heavy burden on the whole country as well as on the alluvial territory, and no positive or lasting good will be secured. And it would appear that, before proceeding further with the undertaking, a very careful consideration should be given the matter, and an endeavor made to secure, if possible, some less expensive, more satisfactory and efficient method of protecting this valuable section, as also one which will conform better to Nature's laws, and secure for posterity all the advantages and benefits possible.

The problem presented is apparently most intricate from every point of view, and will, perhaps, require most careful and searching investigation and study; but the solution should not be delayed, as the settling up and improving of the territory presents additional complication and trouble, and the quicker we determine upon the proper method to be adopted, the better for all present and future interests.

It is apparent, however, that the intricacies of the solution of the problem lie more in the detail than in the general principle of a system which will accomplish the desired results properly and satisfactorily. By a consideration of the topographical conditions of the territory, in conjunction with what is reasonable to assume would be Nature's methods in handling the problem, if not interfered with by the work of man, together with the experiences, data, and facts secured during the past seventy-five years, a very clear and positive general principle is descried.

Referring to the topography of the territory, it will be observed that the main trunk of the Mississippi, from the mouth of the Ohio to Memphis, is located on the extreme easterly edge of the alluvial embayment, directly against the high bluff territory of a former geological age, and has a general direction almost due south. It will also be observed that, from Memphis, the trunk leaves the higher and older territory and runs southwest diagonally across the alluvial to Arkansas City, when it assumes a southeasterly direction and recrosses the alluvial on a diagonal from Arkansas City to Vicksburg, where it again strikes the higher and older country, on the extreme east of the alluvial.

From Vicksburg to Natchez the river runs southwest; from Natchez to the mouth of the Red River the direction is about due south; and, from the mouth of the Red River to Plaquemine, the direction is southwest. The trunk of the river, from Vicksburg to Plaquemine, hugs the higher and older country lying to the extreme east of the alluvial. From Plaquemine to New Orleans the general direction is but a little south of east, and between these points the trunk keeps well to the north of the alluvial territory as it exists to-day, paralleling the southern limits of the higher and older territory. From New Orleans to the sea the general direction is southeast, and directly through the alluvial. Mr. Brown.

It will be observed that the river hugs the high country on the east side of the alluvial embayment throughout its whole length, from the mouth of the Ohio to near New Orleans, excepting between Memphis and Vicksburg, and that the tendency of the river throughout is to hug or parallel this higher or older territory on the east.

The older geological formation on the east of the alluvial is much higher and more abrupt, where it meets the alluvial, than is this older territory on the west; hence the tributaries entering the trunk from the east have sufficient velocity and abrading force to cut a waterway through the alluvial banks of the trunk; and the formation of the older territory has more resistance against abrasion than the alluvial, and hence provides a waterway of more stability. A further cause is due to the fact that the sediment of an alluvial stream which deposits and raises the banks and the surrounding country on both sides of the stream will in time provide a cause for the change of the bed or trunk of the stream by the abrading and tearing down of the banks on that side of the stream which receives tributaries which have sufficient power to abrade and tear down, as, for the reasons mentioned, the tributaries from the east no doubt have.

That at a former period of time the trunk of the Mississippi was far to the west of its existing location is shown clearly by the existence of the high ridges lying between the western edge of the alluvial embayment and the present trunk, or, perhaps there were at times several well-defined trunks.

The existing topography would indicate clearly that at one time the Red River discharged into the sea, perhaps as far west as Cote Blanche Bay; and when we consider that the westerly and southerly banks of the streams—whether one or more—which existed previously in the alluvial territory were subjected to less abrasion and wear from tributaries than were the easterly and northerly banks, the cause for the present location of the trunk is clearly shown.

Had the alluvial basin of the Mississippi not been inhabited for centuries to come, and had Nature been allowed to finish the work in her own way, there is no doubt that eventually there would have been

Mr. Brown. two large separate and distinct rivers, extending from about Cape Girardeau—or from a point above the mouth of the Ohio—to the sea; one directly adjacent to the old and high territory on the extreme east and north of the alluvial embayment, which could receive and deliver to the sea all the waters from the eastern tributaries, and one on the extreme west of the embayment, receiving and discharging all the waters from the western tributaries.

The process of the formation of these rivers would be that the trunk as now existing would be gradually moved, by the constant abrasion and tearing down of deposits on the eastern and northern banks by the tributaries from the east, until it occupied a position directly against the higher, older and more stable formation forming the eastern and northern boundary of the alluvial; and the trunk, from Memphis to Vicksburg, would probably pass in the neighborhood of the present towns of Charleston, Greenwood and Yazoo City, in the State of Mississippi; and, from below Baton Rouge, the river would flow through Lakes Marapau and Pontchartrain and discharge into Mississippi Sound through the territory between the Rigolets and Chef-Menteur.

The length of the river by this route, from Baton Rouge to Mississippi Sound—tide water—would be about 95 miles. From Baton Rouge to the sea by the present trunk is 220 miles, or 125 miles further than the route which Nature no doubt proposed to adopt ultimately.

When this eastern main trunk had become permanently located directly against the older and more stable formation on the east; and the west bank of the stream had become in time sufficiently elevated, from the annual deposits of an alluvial river, to exclude the waters from the west, these waters from the west would seek and cut a channel in the alluvial on the west of the main trunk, and begin to discharge directly to the sea, entirely independent of the main trunk located adjacent to the eastern shore of the alluvial; and this independent stream, conveying the waters from the west to the sea, would be moved westward gradually until it occupied a location directly adjacent to the older lands on the western edge of the alluvial.

The process of moving this second stream, and the cause, would be the same as that which had moved the trunk to the extreme eastern limit of the alluvial. The western bank of the second stream would be constantly torn and abraded by the waters from the western tributaries, and the ultimate delta of this second river connecting with sea would no doubt be as far west as the western limit of the alluvial embayment—about at Cote Blanche Bay.

The alluvial territory between these two streams, at the time when they were ultimately developed, would be well filled, and high, with lakes and drainage branches and a drain directly to the sea entirely independent of the two main rivers.

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The study of the subject, on the basis of carrying out practically the principle adopted by Nature, with such modifications as will reasonably meet the conditions of the alluvial as they now exist, would perhaps develop a measure by which the low swamp regions of the territory would be utilized by connecting them, where necessary, by cutting through the ridges, and thus provide a secondary stream to carry the surplus water direct to the sea.

The high land surrounding these low swamp places could be protected by levees, and this secondary stream could extend from near the junction of the Ohio to the sea, the main trunk of the river being arranged so as to deliver all flow above a certain stage direct into this secondary stream.

Assuming that the future floods will aggregate a maximum flow of 1 500 000 cu. ft. per second, it is reasonable to assume that the main trunk will discharge, for a century to come, without any large additional improvement, 900 000 cu. ft. per second, at a flood elevation not exceeding that of 1874. The maximum volume to be delivered by this secondary stream would be 600 000 cu. ft. per second; and if this stream were two miles wide, with an average depth of 9 ft., it would discharge this volume with a velocity of about 6 ft.

This secondary stream, to embrace the full area of the low swamp territory, would have an average width largely exceeding two miles, and the width through the ridges would be made to suit the conditions.

Cross-levees, forming reservoirs, could be constructed where desired, and outlets from the stream, to reservoirs on either side, could be constructed as might be found expedient. An opportunity would thus be offered for the retention of as much water as might be desired for industrial and agricultural purposes, and the swamps would be gradually filled. The volumes retained in this secondary stream as a reservoir, and in the special reservoirs, would reduce materially this maximum aggregate volume of 600 000 cu. ft. per second.

To describe the measure in more detail, yet in a general way, it is suggested to provide a secondary stream by leveeing in all the swamp territory lying west of the existing trunk, and extending from near Dog Tooth Bend, above Cairo, to a point near Helena. This swamp embraces the Hatchie Coon sunken lands, Roaring River and St. Frances Lake. The secondary stream should be connected with the river by suitable waste weirs, of proper proportions, near Dog Tooth Bend, and also at such other points as might be found necessary, as at New Madrid and Centennial Lake by cross-levees; which latter, as outlets, would admit of filling the extensive low swamp areas embracing Lake Nicormy, Big Lake and Tyronza Lake, when the adjacent high land was protected by levees.

This secondary river should enter the main trunk near Helena. From Helena, the secondary stream should be continued on the east side

Mr. Brown. of the main trunk from near Helena to near Vicksburg, through the low swamp lands embracing either the Big Sunflower or the Yazoo Rivers. The levees embracing these swamps should be located so as to protect properly all the adjacent high and improved territory.

From Vicksburg, the secondary stream should be continued on the west side of the main trunk, from a point near Vicksburg to the Red River, and would be formed by the leveeing of the large swamp areas embracing the Tensas and Black Rivers.

From the Red River to near New Orleans it is suggested to improve the trunk of the river so as to deliver an approximate maximum of 1 300 000 cu. ft. per second, for use in filling up the large areas of low swamp lands east of the Lafourche. The waste weir at the Red River should be constructed so as to deliver the surplus into the immense swamps bordering the Atchafalaya River. All the high land surrounding these swamps should be properly protected by levees, the surplus water finding its way to the sea over the extensive marsh bordering Atchafalaya Bay.

All the high lands bordering the large swamp areas east of the Lafourche should be properly protected by levees, and these swamps connected with the main trunk of the river by a suitable waste weir.

A waste weir of proper proportions and construction should be located on the east bank of the river, near Grand View Reach, and connected by cross-levees with Lake Marapau.

The object of connecting the large swamps east of the Lafourche, and those surrounding Lake Marapau, would be to fill them gradually, and in course of time render them of value for cultivation.

The locations referred to in the foregoing outlined description are merely to serve as explanatory of the proposed system. The determination of the details, as to the location of relief works, or of the secondary stream, or of making this secondary stream entirely independent of the main trunk, instead of being connected as described, would be determined by the conditions developed from a thorough survey and investigation of the territory.

The drainage of the high and improved territory, under the conditions resulting from the method of protection outlined herein, would, of necessity, require to be artificial, which, as a matter of fact, where any proper drainage exists, is that under existing conditions.

The effective and economic construction of the levees to confine the waters in the swamps and low places, throughout the territory, would be by floating dipper dredges, which, in the formation of levees, would construct proper drainage canals to serve the adjacent high and improved territory, and provide proper and economical service to all interests. The drainage of the territory should be designed and operated by the same authority which controls the protection system, and thus be made part of the protection work.

The protection of the alluvial lands from inundation, by a system

which will relieve the main trunk of the excessively high flood planes and accompanying high velocities and other conditions which are detrimental to, and incongruous with, a permanent channel location, will improve most materially the navigation of the stream, and will admit of some reasonably satisfactory and efficient results being secured from the construction of works for that purpose. It will also admit of such works being maintained in position to serve the object for which they were constructed. Mr. Brown.

The construction of the levees on the main trunk of the river, on such lines as will admit of a more uniform cross-section for flood volumes than is now provided, and the clearing of all the batture areas, between levee lines, of trees and undergrowth, which interfere with the free flow of the water, and the maintaining of this water section free from obstructions, will increase the flood discharge, and also assist most materially in maintaining a permanent channel, which will thereby improve the navigation of the stream.

The momentous advantages sought to be secured are:

1st.—Reduction of the flood plane;

2d.—Reduction of the dimensions of levees;

3d.—Reduction of flood velocities in the main trunk;

4th.—The formation of a more stable bed for the main trunk, thereby avoiding the abrasion of the high banks;

5th.—The reduction of the fouling of the bottom and sides of the main trunk;

6th.—The maintenance of a reasonably permanent channel, thus improving the navigation of the stream;

7th.—The gradual filling of the swamps and low places;

8th.—The improvement of the health of the country;

9th.—The retention of all water necessary for industrial and agricultural purposes;

10th.—The economy and efficiency of all works constructed for the protection of the territory, and for the navigation of the river;

11th.—The positive protection of all the improved property against inundation.

When the subject of the protection of alluvial lands is forced upon the civil engineer by dire calamity, resulting from the failure of protection works, he is attracted by its great importance, and even in giving a passing thought to the subject, he is astonished and amazed. The consideration of the many combinations of varied conditions existing in the vast territory tributary to the Mississippi, coupled with probable future meteorological and climatic changes, and the consideration of the alluvial embayment as it was in times past, as it is in this age, and as it will be in future ages—or would have been had it not been inhabited—provides material for deep reflection, and causes man, even in this age of great engineering attainments, to pause before as-

Mr. Brown. suming a responsibility in connection with a work which will affect all future ages. Thus the very important question is presented: Shall the present system, confining all the flood waters in one trunk, which entails disastrous results to all present and future interests, be continued in opposition to Nature's laws; or shall our works of protection be modified so as to carry out the design of Nature in a reasonable way, thus securing the greatest possible benefit to all interests of this and of future ages?

Mr. Ockerson. J. A. OCKERSON, M. Am. Soc. C. E. (by letter).—In early days, prior to the advent of the levee system, the steamboat-man and the passenger going down the Mississippi River saw a narrow strip of cultivated land along the immediate banks of the river. They did not realize that for forty to sixty miles beyond that strip the alluvial basin was practically uninhabited and its rich soil untilled.

They saw the fields covered with water during flood times, to a depth of perhaps 3 or 4 ft., and very limited areas in certain localities, developed by radical changes in the regimen of the river, were known to be above water except during extraordinary floods. They did not appreciate the fact that perhaps five miles farther back the water was 10 ft., or more, in depth; that, without levees to control the floods, these great interior basins could not be inhabited or cultivated.

In the meantime systematic levee work began, and, year by year, the levees are gradually being brought up to such height as will finally effectually carry the greatest floods safely to the sea. These same men notice that the flood water, on the battures and the lands between the levees, gradually becomes deeper and the levees grow higher, and they conclude that if the levees can ever be made to control the floods at all they will ultimately "reach the tree tops in height." They are fully convinced and most positive in their opinion that the increase in flood height is due to the raising of the river bed.

To the engineer, it is no surprise that there should be an increase in the height of a flood confined between levees from one to five miles apart over that of a flood confined only by the hills that limit the basins with a width of forty to sixty miles. More than that, the engineer, in the beginning of the work, computed the heights which the maximum confined flood would ultimately reach, and the results have shown that his calculations are very near the mark.

Everyone familiar, even to a slight degree, with the physical characteristics of the river, has noticed the extraordinary local changes that occur in brief intervals of time. Very few, however, realize the fact that tangible changes in the general regimen of the river require long periods of time.

The belief, among laymen, that there is a general progressive elevation of the bed of the stream going on, which is augmented by levees, is widespread. Statements have been made, by those who ought to

know better, that the bed of the river at New Orleans is higher than the adjacent land, while the fact is, that the bed is some 200 ft. below the land. Mr. Ockerson.

A former Secretary of War, in discussing this question with the writer, stated that he proposed to settle the vexing question himself by "measuring the river in several places." Just what he intended to compare the measurements with, or how he proposed to eliminate the effect of purely local changes, does not appear.

The statement that the Mississippi River Commission had already made many thousands of such measurements, covering some 425 miles of river, may have had something to do with the abandonment of his project.

Without any preconceived theory to prove, and with a view of simply ascertaining the facts in the case, the writer prepared in 1894 a project for a resurvey of the river from the mouth of the Arkansas River to Donaldsonville—a distance of 425 miles—and this project was approved by the Mississippi River Commission.

The first general survey had been made, much of it under the personal supervision of the writer, some twelve to fifteen years prior to that time. This first survey comprised accurate lines of levels, with established bench-marks at intervals of three miles. Each line of soundings across the river (at frequent intervals), had its water-surface elevation determined by levels, hence an accurate cross-section of the bed could be plotted. Surveys made at the later date, referred to the same bench-marks and the same datum, gave reliable data from which to determine the difference in conditions at the two epochs.

A careful comparison of the two would, of course, disclose any general changes of considerable magnitude in the elevation or capacity of the bed.

The thousands of cross-sections of the two surveys were carefully plotted, their respective areas measured, and their mean and maximum depths determined. Then comparisons were made between individual sections and between corresponding groups of sections comprising successive pools and crossings. All this entailed an enormous amount of painstaking work, and the conclusions are as follows:

The crests of the low-water bars, as well as the high-water bars, were found to be lower. About half of the total length resurveyed showed a depression of the thalweg, and about an equal amount showed a slight elevation confined chiefly to the pools.

The results reached by this investigation are not as specific as might be desired, but it does not seem possible that such great elevations of bed as would be required to account for increased flood heights could escape detection.

Embracing, as it did, a comparison of 2 768 cross-sections of the

Mr. Ockerson. river, together with something like 150 000 elevations, it seems to prove beyond a reasonable doubt, if any such proof be really needed, that the elevation of a confined flood in the Mississippi is not due to the elevation of the bed of the river.

There is a still more simple proof which should be satisfactory to all, even if highly prejudiced against a levee system.

Gauges are established at intervals along the river, and are connected with several permanent bench-marks in the immediate vicinity. Frequent inspections keep the gauge zeros at the same height from year to year. The readings are taken by reliable observers, both morning and evening, and a continuous record is thus maintained throughout both high and low waters.

These records show in the most positive way that the low waters of recent years are several feet lower than those of earlier years, with equal volume of flow and with equal channel depths.

Only one explanation of this condition is possible. It points unerringly to a depression of the bed of the stream, and should effectually set at rest any fears that there is such a thing as a tangible progressive elevation of the river bed.

During the height of the flood of 1903 the Mississippi River Commission viewed the river from St. Louis to the Gulf of Mexico. Anyone who could have seen, as they did, hundreds of miles of levee intact, the farmers behind them busy plowing and planting, the fruit trees in bloom, the stock fattening on the green herbage, would surely have been impressed with the efficacy, the necessity, of levees.

Add to this the knowledge that the levee system had served to fill with thrifty settlers the fertile basin where life without levees would be impossible, and it becomes incomprehensible how anyone can oppose the completion of the levee system, unless it be on the score of ignorance as to the facts in the case.

Contrast the peaceful condition of things where levee protection exists with the suffering and misery during a flood along the unleveed portions of the river, as revealed by a trip along the river during any flood, and no argument is needed to demonstrate the wisdom of a perfected levee system.

That occasional breaks should occur in levees, partially completed as to both height and section, is by no means surprising. They are to be expected, and they may occur under great stress on rare occasions even in a completed system. But the area flooded and the damage done on such occasions will be trivial, as compared to that of the general flooding of the entire basins under the "no levee" system.

The argument that the floods should be permitted to fill the basins in order that the sediment might build them up, so as to reach ultimately a height above overflow, has no substantial facts to justify it.

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If it were practicable to deposit on the land all the sediment carried by the stream, it would still take a very great number of years to raise the general elevation of the 30 000 sq. miles of basins to any tangible extent. Then, too, the deposit could never reach the height of the rare, exceptional floods; and, if the doctrine is true that the bed of the river is rising, the relative height of bed, banks and flood would remain the same, and overflows would always continue.

There can be no reasonable doubt as to the possibility of constructing an effective levee system. Far more elements of uncertainty are involved in many engineering problems that have been carried to successful completion.

The engineers of this progressive age will not falter in their conviction that the floods of even the mighty Mississippi can be effectually controlled, and it is not likely that a nation with such great resources as ours, which has undertaken to make the deserts blossom, will hesitate to contribute generously to a project which has for its object the conversion of the vast alluvial basins into fertile fields tilled by a prosperous people, happy and contented in homes of plenty.

Mr Haupt.

LEWIS M. HAUPT, M. Am. Soc. C. E. (by letter).—In this topic, the question is asked "Is the levee theory justified by experience" on the Mississippi River?

This query recurs with each successive high flood, and the river enters its solemn and devastating protest by breaching its ramparts and distributing its surplus where Nature designed it to be deposited. In this respect, the Mississippi Valley does not differ from any of the alluvial plains of the world, and it will be found more satisfactory to study the lessons of their experience rather than spend large sums in experimentation on works which; after all, may not accomplish their purpose. Some of these works of the ancients have been in existence for millenniums, and their magnitude and success have excited the admiration of all civilized nations.

Before referring to them, however, it may be well to state briefly the general operation of the physical law which governs the movement of a great river on its course to the sea.

Celestial and terrestrial gravitation are the upper and the nether millstones which grind the fertilizers stored in the mountain fastnesses, and feed them to the hydraulic conveyors to be transported to the alluvial plains for their reclamation and enrichment.

In the Divine economy, the rivers appear to be the agents for the extension of the habitable regions of the earth, by means of floods. In this process of hydraulic grading and filling, the heavier portions of the silt are deposited near the banks, so that all such rivers are found to run on a ridge and to drain away into lower swamp lands beyond. In the human economy, man occupies the batture along the river banks, and, to protect his property and life, attempts to hold

Mr. Haupt. back the floods by dikes. When this system of defence becomes so general as to cut off the flood waters from the back lands they must cease to fill and must remain stagnant, while all the sediment must remain in the main trunk of the drainage system, and, as the slope and velocity grow less on approaching the mouth, a large portion of the load is lodged permanently in the bed of the stream.

In a state of Nature, the river builds up its mean and low-water bed, and provides for the depositing of its surplus mud and water on the outlying flats bordering the stream. Wherever man has interrupted this process, without providing for a substitute by a supply of water and fertilizer, the garden spots of the earth have been converted into deserts, in which case there is no need for a navigable channel to carry away the products.

The vital importance of the irrigation of the bottom lands is well established by the history of Egypt, the arrogance of whose kings led to the destruction of the reservoirs and canals and the debasing of the country to a condition of servitude, as stated in the prophesies of Ezekiel.*

"Speak, and say, Thus saith the Lord God; Behold, I am against thee, Pharaoh king of Egypt, the great dragon that lieth in the midst of his rivers, which hath said, My river is mine own, and I have made it for myself. * * * Behold, I will bring a sword upon thee, and cut off man and beast out of thee. And the land of Egypt shall be desolate and waste. * * * Behold, I will give the land of Egypt unto Nebuchadrezzar king of Babylon; and he shall take her multitude, and take her spoil, and take her prey; and it shall be the wages for his army. * * * Because thou hast lifted up thyself in height * * * I have therefore delivered him into the hand of the mighty one of the heathen; * * * upon the mountains and in all the valleys his branches are fallen, and his boughs are broken by all the rivers of the land; * * * To the end that none of all the trees by the waters exalt themselves for their height, neither shoot up their top among the thick boughs, neither their trees stand up in their height, all that drink water; for they are all delivered unto death; * * * I covered the deep for him, and I restrained the floods thereof, and the great waters were stayed: and I caused Lebanon to mourn for him, and all the trees of the field fainted for him. * * * This is Pharaoh and all his multitude, saith the Lord God."

Thus, by the restraining of the floods and the withholding of the waters were the haughty nations destroyed by those heathen kings whose hydraulic works had made them powerful.

The source of Egyptian prowess is also beautifully set forth by the prophet when he says:

"The waters made him great, the deep set him up on high with her rivers running round about his plants, and sent out her little rivers unto all the trees of the field. Therefore his height was exalted above all the trees of the field, and his boughs were multiplied, and his branches became long because of the multitude of waters, when he shot

* 29th Chapter.

forth. * * * and under his shadow dwelt all great nations. Thus was he fair in his greatness, in the length of his branches: for his root was by great waters." Mr. Haupt.

In this great country of ours we find the striking paradox of one of the executive departments straining every nerve to store and irrigate the soil, while another department is contending with a mighty river for its total exclusion, yet both having the same purpose in view, namely, that the earth may bring forth more abundantly.

The lessons of history teach that the total exclusion of the flood waters results in a devastation far greater and more disastrous than the temporary damages from the floods, and that in the non-irrigated portions of India and China human beings are dying from starvation, because of the confining of the waters. The question is not a local one, and should be considered carefully with reference to its far-reaching effects on posterity. The system in use on the Nile during its period of great prosperity was not the levee system, which excluded the river waters from the neighboring farms; but the basin or reservoir system, which caught and stored the excess waters of the floods, and, by regulating sluices, passed them from one pool to another along the banks of the stream, and finally returned them to the trough after their beneficent work was done, and without deterioration to the navigation of the river.

The importance of a full understanding of this experience will doubtless justify a few extracts from the records of the early systems used in the Orient in the days of the Pharaohs.

In a recent lecture, before the Khedivial Geographical Society, at Cairo (1903), Sir William Willcocks said:

"The advisability of Egypt undertaking these reservoir works without any loss of time has been well brought before me by a letter just received from Professor Sayce, in which he says: 'As you know, archæological facts have convinced me that * * * the body of water in the Nile is considerably less than it was only 2 000 years ago, when huge boats could be navigated at the period of low Nile; and the destruction of the great forests of Central Africa by the inroads of western civilization will further diminish it. Lake Chad and the dried-up lakes connected with it are ominous reminders of what can happen when forests are cleared away.'"

In describing the modern system of "perennial" irrigation now in use in Egypt, Sir William sounds a note of warning, to the effect that it can only survive during times of peace, and by constant care, while the ancient or reservoir system was as enduring as the pyramids, and survived neglect.

He says:

"Perennial irrigation is no hardy plant like basin irrigation, it needs perpetual and constant attention; it is typical of the structures of our days, the dams and weirs on the Nile, which need unremitting toil and observation to preserve them from destruction. The basin irrigation of the ancient Egyptians depended entirely on the flood

Mr. Haupt. waters of the lateral tributaries, as the Atbara, the Blue Nile and the Sobat, which no engineering effort could turn out of the Nile Valley. The perennial irrigation of our day is chiefly dependent on the summer supplies of the great equatorial lakes and other reservoirs at the head of the drainage system. * * *

"Under the old Pharaonic system of basin irrigation, the Nile flood was the Ministry of Agriculture, and the Irrigation Department sufficed to supply all the requirements of the country. Under the modern system of perennial irrigation the Irrigation Department does only half the work."

As a protection against disastrous inundations, he adds:

"Fifty years hence we shall find the country protected from high floods by escapes into the deserts. * * * Such an escape could be constructed for £3 000 000."

The Nile flood will have been maintained at a level sufficiently high to allow of the utilization of the water-power to its utmost limits, or 150 000 H.-P. for storing electricity and providing perennial irrigation by pumpage for other high tracts of 50 000 acres.

The whole process of the successful and safe treatment of the reclamation works of Egypt is stated briefly in a paper prepared for the Glasgow International Congress, in October, 1902, in which Sir William said:

"If we cast our view back to the dawn of Egyptian history, we can picture the Nile Valley as consisting of arid plains, sand dunes and marshy jungles, with reclaimed enclosures on the highest lands. Every eight or ten years the valley was swept by a mighty inundation. We may well imagine with what awe the ancient Egyptians contemplated laying their hands on the great river and saying to it 'thus far and no further.' The seeds of future success lay in the resolve of King Menes' engineers to confine their attention to one bank of the river alone. It was the left bank of the river which history tells us was first reclaimed. A longitudinal dike was run parallel to the stream, and cross-dikes tied it to the Libyan hills. Into these basins or compartments the turbid waters of the flood were led by natural watercourses and artificial canals, and meantime the whole of the right bank and the trough of the river itself were allowed to be swept by the floods. * * *

"In all probability the first six dynasties contented themselves with developing the left bank of the Nile. As, however, the population increased, and with it the demand for new lands, it became necessary to reclaim the right bank as well. The task now was doubly difficult, as the river had to be confined to its own trough. This masterful feat was performed by the Great Pharaohs of the 12th dynasty. They were too well advised to content themselves with repeating on the right bank what Menes had done on the left. By suddenly confining the river they would have exposed the low-lying nomes of Memphis and Lower Egypt to disastrous inundations. To obviate this, they widened and deepened the natural channel which led to the Fayum depression in the Libyan hills, and converted it into a powerful escape to carry off the excess of waters of high floods; and so successful were they in their undertakings that the conversion of the Fayum depression into Lake Mœris was long considered by the ancient world as one

of the greatest wonders. They let the flood into the depression when it was dangerously high and provided for its return to the river when the inundation had come to an end. By this means they insured the lake against being at a high level during a period of flood. The gigantic dikes of entry and exit were only cut in times of emergency, and were reconstructed again at an expense of labor which even Pharaoh considered excessive." Mr. Haupt.

Thus the history of the Nile confirms the projects which have been thus far unsuccessfully urged for the reclamation and navigation of the great river which drains the most productive section of this Republic. It illustrates the need of lateral overflow for fertilization, of subsidiary reservoirs on the banks and tributaries to retain the excess of the floods from the headwaters, or to restrain those of the lower tributaries and of the enlargement of the mouths and outlets of the delta to lower the flood planes of the low country and deepen the channel for navigation, by removing the bars. May we not learn more from precedents than from experiments, and at much less cost? The dredging out of the obstructing bars in the Tyne between Newcastle and the sea has lowered the flood plane at different points from 3 to 9 ft.

Let us review, however, as briefly as may be, some of the statements relating to the Mississippi, to ascertain the facts as presented by the advocates or opponents of various plans of improvement.

The Mississippi River Commission was created by Act of Congress in the year 1879, when the South Pass was opened at its mouth. Its personnel has changed from time to time, but it has been composed of able men having the courage of their convictions, who have collated a large amount of valuable data as to the physics and hydraulics of the river. It has modified its methods, as the exigencies and experience acquired have justified, both locally and generally. It has tried regulation for low stages, revetments and spurs for holding caving banks, levees for floods and reclamation, and finally dredging, for navigation, and the results of this experience have been summed up in concise form in the Report of the Senate Committee, submitted after the severe flood of 1897. This Committee took a large amount of testimony and examined the works carefully, with a view to determine the following points:

1. To determine the causes of the disastrous floods, and how they may be prevented or diminished.

2. If the floods are due to deforestation, how it may be prevented, or whether reservoirs at the headwaters would be beneficial.

3. Whether such reservoirs could be used for irrigation of arid lands.

4. Whether the outlet system is practicable or expedient.

5. Whether the levee system, with jetties to protect the banks, should be continued.

Mr. Haupt.

6. What has been the effect of the jetties at the mouth upon the commerce and navigation of the river?

7. Whether the Mississippi and the Missouri River Commissions should be continued.

8. What legislation is necessary to prevent the enormous destruction of property by floods, and what amount of money should be appropriated for the establishment of systematic improvements and safeguards.

The Committee concluded that the destruction of timber does not tend to cause floods, but rather to diminish than increase the rainfall. That the five large reservoirs on the Upper Mississippi repress, to some extent, the floods above Lake Pepin, and improve the low-water navigation down to that point. A high reservoir on the Missouri above Great Falls would be useful for irrigation, but not for navigation or floods. The Ohio has no adequate reservoir sites.

"The only place where a reservoir basin can be found, proximate and sufficiently large to afford a holding ground at all commensurate with any material or practical relief is the St. Francis Basin; but the cost of constructing and maintaining a system of reservoirs in this basin would be enormous, and far greater than the cost of leveeing the entire river front of the basin. The scheme is regarded by nearly all engineers and other experts as wholly impracticable. In short, your committee can discover no just or adequate relief in reservoirs."

"Neither can it discover any material relief from the outlet system. It is not practicable to relieve the river by means of outlets except below the Red River. The St. Francis, Yazoo, White and Tensas Basins can get no relief from any practicable outlet system, and where this system exists and is feasible there is no disposition to extend it or to substitute it for levee enlargement."*

With reference to the levees, the Committee states that their history has been one of gradual reclamation. Anterior to levee construction these basins served as great natural reservoirs for the floods, which never reached the high level nor the increased rapidity obtained since the great extension of the levee system. Levees are the penning up of the floods of the river from these high-water basins, and the stream at its flood stages becomes higher, necessitating higher and stronger levees. The first levees in Louisiana were but 4 ft. in height; they are now 12 to 13 ft., and this height proved insufficient in the great flood of 1897, when the flood stage above New Orleans was from 1½ to 3½ ft. higher than that of any previous flood, notwithstanding that the flood level at Cairo was less than in 1882, 1883 and 1884. This was partly due to the increase and improvement of levees. The experience of 1897 indicates that a complete inclosure of all the river basins will require levees from 3 to 4 ft. higher in Louisiana.

The levees in the Yazoo Basin, originally only 4 ft. high, were increased to an average of from 13 to 14 ft., prior to 1897, which was more than 3 ft. above the high water of 1882, but it proved "utterly

* Humphrey's and Abbot's report of 1831, as to reservoirs and outlets.

insufficient under the flood of 1897," which made it plain that the complete inclosure of all the basins would require an increase in the Yazoo levees of from 4 to 6 ft. The same is true of the White River Basin, which was overtopped by the flood of 1897 at many points, as it rose from 4 to 6 ft. above any previous flood.* Mr. Haupt.

The upper and lower Tensas Basins were leveed since 1882 to a height of about 13 ft., but the water of 1897 was from 2½ to 3 ft. higher in front of these basins, and the levees were insufficient. The higher flood was largely due to increased levee construction. The completion of the levee system would require the levees in the Tensas Basin to be raised to the height of from 17½ to 18 ft.

Work on the St. Francis front began in 1893. Since that time about 127 miles have been constructed to an average height of 9 ft. The levees proved insufficient for the flood of 1897, and several serious crevasses occurred. They must be made at least 2 ft. higher. The flood of 1897 wrought great havoc. There were twenty-three breaks in the St. Francis front; six in the Yazoo; fourteen in the White; four in the Tensas, and a few below the Red which were unimportant. (It may be well to note here that these unimportant breaks, which were closed during the pendency of the flood, were below the outlet *via* Red River.)

The Committee concludes that the flood of 1897, in its effects and consequences, was greatly enlarged and aggravated by the extensive inclosure of basins and the extended and enlarged levee construction which has taken place since then. It refers to the early report of 1861, made when levees were in their embryonic stage, as authority for the statement that "no substantial relief from the floods could be obtained from reservoirs or outlets, and that levees properly constructed, would afford the necessary relief and protection." Subsequent reports sustained this contention, and, in view of the record and accumulated experience with this system of protection, it is somewhat disconcerting to find the Committee reaffirming the earlier opinions to the effect that "the river can only be protected and preserved from such floods by an ample and complete system of levees from Cairo to the head of the Passes." It adds, crevasses are liable to occur as long as the system is incomplete; and it was then (1897) estimated that it could be done at a cost of from $18 000 000 to $20 000 000 in from four to five years. That time has elapsed, and the flood of 1903 has added its testimony to the inefficiency of the protection works, and shows no substantial gain as to increase of depths or permanency of channel.

A few citations from residents and engineers on the Mississippi may serve to reveal the true status of the question.†

* To add 6 ft. to a 14-ft. levee doubles its volume and cost, increases its unit pressure, length of saturation, and hence its danger of caving.

† "Riparian Lands of the Mississippi River," by Tompkins.

Mr. Haupt. A member of Congress from Mississippi says:

"The levee system being very much stronger and more extensive than ever before, the flood elevation all along the river was greater than during any previous floods. This, of course, was to be expected."

On the other hand, an engineer of long experience stated:

"Having the St. Francis Basin closed cannot raise the high-water line one foot at Helena but will doubtless restrain it slightly, which reduction will continue as the shoals are scoured out by the restrained floods."

He does not explain how these shoals get out of the river's bed.

Still another authority states that:

"Since 1882, with the growth of the levee system and the gradual elimination of reservoirs, and contraction of the high-water sections, the flood plane had risen, each high water, for a given volume of discharge, giving greater gauge readings."

A comparison of readings showed that the flood of 1897, although 0.3 ft. lower at Cairo, was 5.6 ft. higher at Helena; 5.1 ft. higher at Greenville; 2.0 ft. higher at Natchez; 1.7 ft. higher at Red River (where there is an outlet); 4.7 ft. higher at Baton Rouge, etc., thus leaving no doubt in this instance of the increased height due to the additional contraction of the waterway and the lowering of the head relatively by the existence of an outlet.

The effects due to the later extensions may be seen by reference to the unofficial reports of the flood of March, 1903.

On the 20th of that month, the gauge at Memphis, abreast of the St. Francis Basin, registered 40.1 ft., and seven days later a crevasse occurred at Hollybury, 20 miles north, which in one night lowered it to 39.4 ft., as compared with 37.7 ft. in 1897, the highest previous record.

At Greenville, Miss., the water was reported to be 4 ft. above any previous record, and rising at the rate of a half a foot per diem. A week later a "three-mile crevasse" was reported south of that place, said to be the worst ever experienced.

At New Orleans, on March 28th, it was reported to be 20.2 ft., or from 0.6 to 0.7 ft. higher than all former records, but, fortunately, the tension was relieved by a crevasse at Hymelia, about 40 miles above the city, which was not closed during the flood.

The record leaves no doubt that as the levees are extended and the flood waters withdrawn from overflow, the stage rises and the dangers resulting from crevasses still remain as a menace. The completion of the system is expected to establish a limit to the height of the embankments, but this expectation is based upon the assumption that all the sediment carried into the trunk by all its tributaries must be ejected at its mouth, as the natural deposit over the banks has been cut off. Even if this were a physical possibility, which it is not, for the reasons previously stated, the rapid extension of the delta

by the enormous deposits would continue to retard the discharge and raise the flood plane, requiring constant accretions to the section all along the line to prevent overtopping. But this is not the only source of danger or expense. The erosive action of this great torrent at the bends, and especially at the contractions, incidental to the great variations in cross-section, is an ever-present source of danger, causing caving of the banks and their superimposed levees, which it is estimated goes on at the average rate of 9 acres per annum for each mile between Cairo and the mouth of the Red River, and this risk increases with the height, because of the increased head, saturation and duration of the higher water. It is asserted by engineers of experience on the river that the bed is not rising, although they state that the contrary opinion is widely prevalent; but, when asked for a reason, those who believe it is "can give none except, perhaps, of a fantastic sort." It remains for those who deny it to account for the disposition of the large accretions fed to the river by its tributaries when the natural dump is cut off. This is attempted to be explained by the statement that the conditions indicate that "the alluvial agencies have nearly reached a state of equilibrium, and the further progress of the filling-up movement is exceedingly slow." This recognizes, but minimizes, the action as negligible. Mr. Haupt.

The revetment of caving banks has been tried extensively, but is now almost abandoned. The testimony is to the effect that:

"The revetment idea at first inspired some hope, but it was soon seen that its cost and want of permanency would confine it to a few points. It was therefore with much satisfaction that we saw the dredging and portable dike idea introduced."

Another engineer writes:

"The absolute failure of mattress work to protect abrading banks has been very thoroughly and positively demonstrated."

Another states that:

"The use of contraction works has been abandoned to a great extent, and the general improvement of the river by bank protection, revetments, spurs, etc., has been postponed until some less expensive structures are designed or discovered. * * * The low-water channels are being maintained by dredging during the low-water season. * * * The St. Francis and Yazoo are of little consequence, except in that their basins act as relief areas or reservoirs during high stages of the river."

Such, then, are but a few of the many statements and opinions concerning the experience and results secured on this great river, from which it would appear that the end is not yet, and that important remedial measures have been brushed aside without the consideration which their possibilities demand.

Outlets.

It is an axiom that if the outflow from a vessel be made equal to or greater than the inflow there can be no overflow, and yet the applica-

Mr. Haupt. tion of this simple expedient is condemned as being injurious, impracticable, or too expensive, and the eminent authorities of the earlier investigations are cited as being opposed to this method of relief. So important is it that a brief consideration appears to be warranted by reference to the original sources.

The injurious effect of outlets is generally stated to arise from:

"The escape of water from the channel, which, under any circumstances, is accompanied by a reduction of velocity below the point of escape, and which thus creates deposits in the channel and dams the discharge."

Another says:

"Outlets will induce a shallowing of the river and a raising of the bed, and consequently of the water line. In short, the only one of the remedies above mentioned that is satisfactory to those concerned, or that has had any extensive application, is the method of artificial embankments or levees."

This statement, that the outlet system has had no extensive application, would indicate that it is purely theoretical, although the natural outlets amply demonstrate the claims of their supporters, as will presently appear. The Nita crevasse of 1890, 60 miles above New Orleans, is cited as an evidence that the outlet system is useless as a measure of relief. This is merely a question of interpretation. What were the facts?

"This crevasse was 3 000 ft. wide and 15 ft. deep. It discharged 400 000 cu. ft. per second, or 30% of the flow, as measured just above the crevasse. The depression of the flood surface immediately below was 1½ ft. At New Orleans it was 1 ft. At Plaquemine, 50 miles above, it had no effect at all."

From this instance it was concluded that:

"Outlets must disappoint the expectations of their advocates, as was noted by the able pioneers in this subject, Generals Humphreys and Abbot, who, lacking actual measurements, supported the proposition by a very ingenious method of indirect proof, which showed in part, but not completely, the futility of outlets as a means of flood relief."

On the contrary, the Nita data prove that the outlet lowered the surface of the river for at least 100 miles, by from 1 to 1½ ft., and saved the city of New Orleans. That it did not lower it 50 miles above, is accounted for by the fact that the supply from the higher reaches was such as to maintain the stage which the slope and sinuosities of the bed for the 50 miles below prevented it from voiding. Had not the breach occurred, the stage at New Orleans would have been at least 1 ft. higher. It was flush with the crest when the crevasse occurred. It was therefore a positive measure of relief to the entire river below the break, and for some distance above, and its injurious effects on the stream, if any, remain to be discovered. So, with the many other large crevasses which have occurred in the stretch between the Red River outlet and the mouth, where, in consequence of the many outlets, the

stage is more constant than in any other section of the river below St. Louis, and the navigation is most satisfactory. Mr. Haupt.

A distinguished member of the Mississippi River Commission* thus describes this outlet section:

"In this portion of the river the channel is narrow, averaging about half a mile in width, and the depth sometimes exceeds 200 ft. Bank erosion is slight as compared with the reach above, sand bars, as obstructions to navigation, are almost unknown, and neither contraction works nor dredging are required. Here Nature has constructed an ideal channel, with depths at all times sufficient for the largest seagoing craft. For nearly the entire length (310 miles) the water is confined to a single channel, down to the head of the Passes, there being but two islands in the entire reach. As the upper limit of the reach at low water is less than 2 ft. above mean Gulf level, it often happens that the tidal effect is noticeable throughout its entire length. There are several outlets through which the waters of the river can flow to the Gulf of Mexico. Some have been closed by levees, but others are still open. At the head of this reach is the Atchafalaya, which is the first practicable outlet to the sea. * * * The extreme oscillation in stage recorded up to the present time at New Orleans is 20.7 ft."

Such, then, are the characteristics of this outlet section of the river, with its minimum of fluctuations, deepest water, fewest bars, least erosion, and best channel, all of which may be ascribed to that one feature which all the sections above lack, namely outlets; and yet it is claimed that the outlet propaganda is merely a fanciful theory, not based on experience. What would become of the country if there were no outlets? This is an extreme case, it is true, but every obstacle at the mouth of a river contributes in some measure to dam it up and raise the level by reduced slope and velocity.

With reference to the "claim that actual measurements confirm the opinion that outlets must occasion deposits in the channel," Humphreys and Abbot say that, after a careful investigation of the sections at certain crevasses, these claims "fall to the ground." They also add that an extended series of measurements was made with especial reference to testing the assumptions upon which are based the conclusions that outlets will raise the mean level of the bed of the Mississippi, and "they have demonstrated both to be erroneous."

Speaking of outlets, they say emphatically:

"The conclusion is then inevitable, that, so far as the river itself is concerned, they are of great utility. Few practical problems admit of so positive a solution."

They then proceed to consider the various channels whereby the flood waters might be conveyed to the Gulf, and appear to have been deterred from recommending them because of what appeared to them, at that time, to be the great cost of such relief measures, and the local injury to the rear of some low-lying plantations.

*J. A. Ockerson, M. Am. Soc. C. E.

Mr. Haupt. It is stated that the distance between the river and Lake Pontchartrain is only six miles, and the fall is 19.6 ft. at high water (1851).

"There can be no doubt that by making two levees from the river to the lake a high-water outlet of any dimensions can be made. Such an outlet, they say, would be of utility in reducing the height of the floods for many miles above and below, but its construction would be followed by consequences disastrous to Louisiana."

What these disastrous consequences were, which outweighed the benefits of reduced floods in 1861, are thus set forth: (1) the works must be difficult and costly; (2) the navigation of the lake will be rapidly destroyed; (3) there is danger that the outlet will become a main branch of the river; and (4) the navigation of the present mouths be thus seriously impaired.

With a new and shorter outlet to the sea, when the ruling depths by the delta mouths were wholly inadequate, and with the navigation of the lake improved by the better channel to be created, it is difficult to understand how such works would have proved disastrous to Louisiana.

In considering the length of time required to destroy the navigation of the lake, it was assumed that the river discharged a volume of sediment equal to 1 sq. mile, 21 ft. deep, each year; and at this rate it would require 375 years to fill the lake, if it were all deposited in it, which could not be expected. But, even then, it does not appear that the filling up of a non-productive lake and the reclamation of 600 sq. miles of the most fertile land on the globe would be such a serious disaster to the State, while the value of that land would doubtless pay many times over for the cost of the works, so that the outlet would accomplish precisely what the levees are designed to do, namely, reclaim more land than it would destroy. The lowering of the flood plane throughout this section would also add largely to the available productive territory by a very simple and expeditious engineering work, which has been urged upon the attention of the Government for some years past, but apparently to no purpose.

After a careful review of this classic report, the writer is unable to find any other or more weighty reasons assigned by its writers for the conclusion which they reached, that "outlets are not advisable." It would seem to be unfortunate for the welfare of the country as a whole, and for Louisiana in particular, that this view should have become so deeply grounded. The river crevasses and the outlets of to-day tell a different story, according to some interpreters. As outlets are best for the river, and it is practicable to relieve it, not only at Lake Pontchartrain and Lake Borgne, but at other points, at a reasonable cost, and to reclaim more arable land by so doing, they would seem to be fully justified and worthy of being inaugurated.

To avoid any possible misconception, the writer desires to state that he does not condemn levees as an auxiliary device to assist in con-

trolling the flood movements, and that by outlets, he does not mean the cutting of great natural breaches in the dikes, but suitably arranged spillways with sills at proper elevations above ordinary stages and surmounted by movable dams, shutters or gates, for the regulation of the discharge. As far as the practicability of these devices is concerned, there can be no question, as the Government has made an excellent demonstration of their efficiency on the Kanawha and Upper Ohio Rivers and elsewhere. Mr. Haupt.

RESERVOIRS.

Still another source of relief remains to be noted briefly.

At the Convention of this Society held at Old Point Comfort, in 1892, the writer submitted a few suggestions in these words:*

"It would seem practicable to provide a sufficient number of large lateral subsiding basins or lakes by enclosing extensive areas at intervals where the topography admitted of economical construction, into which the flood waters could escape, and in which, the velocity being reduced, a large part of the silt would be precipitated, while, after the passage of the crest of the flood-wave, the clearer waters from the reservoirs would return to the river and become useful for navigation. In short, instead of serving the sole purpose of maintaining the water supply for low stages, * * * they would also reduce the rate of raising the bed by providing lateral dumping grounds outside of the bed of the stream, and would also reduce the flood plane and dangers from inundation. There are numerous places on the river where, by making return dikes extending back to the bluffs, many square miles of land, now of little value, might be utilized for such safety valves for the river, with substantial benefit and at a comparatively small cost.

* * * * *

"The systematic improvement of the river must treat the problem as a whole, and provide at high stages for the rapid emptying of its basin at the outlet, the retardation of its filling by its tributaries, provision for deposition of its load and temporary escape of its excess of water along its course; while for the low-water stages for navigation the stream must be canalized so far as practicable to retain a nearly uniform velocity."

In the discussion of these suggestions, which followed, it was stated that:

"It is not established that general relief is found by using these basins to receive and hold back for a time the water overflowing into them from great floods."

As no such basins have ever been created on the Mississippi, there is no local experience upon which to base this objection, for the natural overflow and run-off are not the conditions contemplated, but a series of reservoirs for which the Senate Committee said this reach was well adapted, the only objection it raised was to the cost, which it thought would be greater than that of leveeing the entire front of the basin. But that is not the proper basis of comparison as to cost,

* *Transactions*, Am. Soc. C. E., Vol. XXVII. p. 302.

Mr. Haupt. since the closure of the basin not only involves the cost of the levees along its front, but the elevation and strengthening of nearly the entire line from Cairo to the sea, compared with which the cost of the reservoir and regulating works would be a small item. In addition thereto, there would result a definite control of floods, a slack-water navigation for more than 300 miles of river, a series of reservoirs for storage and irrigation canals, and the reclamation of much arable land along the banks, all the way down the river. These benefits are sufficient to warrant the construction of the system which has been so fully and carefully described* by James A. Seddon, M. Am. Soc. C. E. In this paper it is stated that "a 20-ft. channel, navigable at all seasons, is shown to be feasible, by the group of professional men best qualified to judge."

The painstaking researches given to this all-absorbing question by one whose life has been spent in these studies, entitle them to great weight, and they reveal the practicability of so regulating the river floods, and at the same time of providing a sufficient channel, as to commend them to the earnest attention of the Government.

It is not reassuring to be told that when the levees are complete the lands behind them will be safe from inundations, for it is only necessary to look to recent experiences in other countries which have taken refuge in long-completed levees. In China, for example, it has been officially reported that the Yangtse Kiang had broken out recently above Nanking, the water reaching the highest stage in fifty years. The devastation was widespread. Villages, farms, crops and live-stock were swept away, and famine stared the people in the face. The loss of life was very great, thousands being drowned, and the natives were living in the trees. Riots and robberies prevailed as a result of suffering from famine. This catastrophe occurred in October, 1901.

In another instance, it was reported that 3 000 000 persons were drowned by the bursting of the levees along the Yellow River, which has been appropriately designated as "China's Sorrow," because of the great destruction wrought by its floods.

With reference to the present experience in Egypt, it is stated that:

"The necessity of constructing levees to exclude the Nile water from the cotton-growing fields has rendered the inundations destructive, and the speculation seems on the whole to have injured the welfare of Egypt."

But there is another feature of the levee system to which attention should be directed before dismissing the subject, and that is the great irregularity in cross-section which it necessarily introduces in consequence of the bends of the stream and the character of the supporting ground, thus violating one of the most important requisites for the permanent improvement of a stream. A mere glance at the maps

* Paper read before the Western Society of Engineers, June 20th, 1900, and reprinted by the Illinois River Valley Association.

will serve to impress this feature of the levees. For example, at Mile 430, below Cairo, the distance across the river between the jetties is about 1 mile; 30 miles higher up it is about 15 miles. Ten miles below it is 5 miles; in 20 miles more it widens to 8 miles, and 5 miles lower contracts to 1 mile. This great irregularity of width necessitates corresponding fluctuations in depths at high water, with high velocities at the gorges and deep erosion resulting in higher shoals and bars in the wide reaches, thus building up obstacles to navigation, as may be illustrated by the detention of the monitor *Arkansas* at St. Genevieve, Mo., during the period of the year when the river is supposed to be navigable for vessels of that limited draft. Mr. Haupt.

In many places the building of a few miles would have saved from five to ten times as many and have given a far better alignment.

Conclusion.

To bring this many-sided question to a conclusion, it may be said that outlets and regulating reservoirs or basins have not had the consideration which their many advantages demand for them, but have been dismissed on theoretical grounds, while the ancient experiences demonstrate them to be the most important factors in contributing to the success of great river regulation, with the result of ultimately reclaiming and protecting more land and rendering it more productive than a system which looks to the almost total exclusion of that element so necessary to agriculture, water. The levees undoubtedly increase in height with the extension of the system, they overload the banks, do not prevent caving, do not regulate the stream, increase the fluctuations between high and low stages, instead of securing a more nearly normal discharge, and are in themselves very expensive to create and maintain, being subject to the direct attack of the flood forces.

The national prosperity is largely dependent upon the maintenance of the great central basin as a granary for domestic and foreign supplies, and hence the question should be carefully and fearlessly considered solely on its merits. It is believed that this can be done to great advantage by the members of this Society, and that the topic is timely and deserving of the fullest consideration.

* * * * * *

The foregoing discussion was prepared prior to the presentation of the discussions at the Convention, and the writer, having read the latter since that time, thinks it necessary to add a few supplementary deductions.

From the discussions by several members of the Mississippi River Commission, it appears that all are agreed as to the ends to be secured, namely, the reclamation of the fertile lands of the Mississippi, but that there are honest differences of opinion as to means, as well as of interpretation of data, which remain to be reconciled.

Mr. Haupt. The Commission's present plan appears to lay stress upon levees as the sole means of reclamation, with incidental channel improvement, supplemented by dredging, and it appears to deny the statements, so generally made and believed, that the effect of the levees would be to cause the bed of the stream to rise; while others, and among them the writer, having full faith in the levees as a partial remedial measure, would supplement them by reservoirs, outlets and other devices, and prefer to take the record of thousands of years as to elevation of bed, to that furnished by surveys covering only a period of some thirteen or fourteen years. However, if Tables Nos. 1 and 2 and the diagram, Fig. 1, prove anything, it would seem that there has been a loss in the low-water mean depth, or an elevation of bed, of about 6%, and in the medium depth of from 6 to 8%, while the bank-full stage of Table No. 2 shows a loss in mean depth of 8.9%, notwithstanding the increase in width in each case. They also show losses of about 3.7% in area for both stretches, at low water; and in 1894 the upper section gained 2.7% in area at medium stage, while the lower section of the river, from Vicksburg south, lost 0.8% at medium stage.

The composite diagram also indicates clearly an elevation of the bottom averaging about 4 ft. (the scale is too small for any accuracy) at low water and an enlargement between low and extreme high water, which enlargement in width does not improve navigation, but represents the amount of detritus fed to the stream by the caving banks, a portion of which is accounted for in the fill at the bottom of the cross-section. The question at once arises as to the disposition of this material eroded from the caving banks and (as determined by planimeter from this small drawing) amounting to 8.6% of the mean cross-section covering 200 miles of river. Since there are supposed to be no outlets whereby it may escape from the stream, there would seem to be no alternative but to expect it to be deposited in its bed or be carried bodily to the Gulf; but it has never been claimed that the deposit at the mouth is equal to the sediment fed to the bed by tributaries and caving banks. Hence the conclusion is inevitable that the bed, as a whole, must rise. This elevation must become more rapid as the area of deposit is confined by levees. The entire alluvial deposit of the basin is the recorded answer of Nature as to bed elevation, and it is illustrated further by the salient spits of land which jet out into the Gulf to support its bed and elevate the waters of the stream above the general level of the Gulf.

The general facts stated in the tables, resulting from the carefully conducted surveys made under the direct supervision of one of the most experienced and competent civil engineers in that service, reveal shoaling of the mean depths at low-water and medium stages, reduction of low-water areas, increase of width and elevation of flood surface,

with large increase of bank erosion between high and low water; it would therefore seem to leave no doubt as to the contention that the bed is rising and the flood-plane with it. The latter is due more especially to contraction between levees than to bed elevation. Mr. Haupt.

Table No. 4 also confirms, as far as such data can, the general experience that each successive high flood, of approximately equivalent volume, has overtopped its predecessor by some feet. In the flood of 1903, the gauge at Cairo was 1.57 ft. lower than the highest previous record, while that at Greenville, Miss., was 2.35 ft. higher than any former record, making a difference (other things being equal) of 3.92 ft. in height, due to levee extension. The same table shows that this flood of 1903, which was not a maximum at Cairo, overtopped the highest previous flood record all the way from Cottonwood Point to the Red River, with but two slight exceptions, while in the outlet section, below Red River, the surface generally was lower than the highest previous records. The profile of this flood, which has not yet been published, will show some very suggestive fluctuations of slope and discharge due to variations of alignment and width between levees.

Already, the menace in the vicinity of Memphis, caused by the partial closure of the St. Francis front, is such as to call for vigorous protests against its extension, since no provision has been made for relief, and it is admitted that:

"A crevasse in the levee of the future will be a more serious disaster than in one of the present time, in proportion to its greater depth and discharge, and the greater improvements which have developed under its protection."

Yet this menace is avoidable, and should be provided for in the scheme of improvement.

The standard levees have been revised frequently as to elevation and section, but, unfortunately, not as to alignment, where the greatest benefits are possible. But little attention appears to have been paid, in these discussions, to the serious irregularities in width between levees, causing pools and chutes, with great irregularity of discharge and slope, bar building and bank caving.

In 1897 it was estimated that the levee system could be completed at a cost of $20 000 000 in from four to five years, yet in the present discussion it is stated that at this date (1903) only 60% of the yardage now considered necessary for the safety of the plantations below their banks is in place, and that the experience of this year "makes advisable a revision of the provisional standard in the vicinity of some of the gauge stations."

Conclusions derived from average gauge readings at a few fixed points should have but little weight, since the shifting of the bars up or down a stream with reference to the location of the gauges, as well as the variation of width between banks, will affect their readings

Mr. Haupt. materially. The increase in width alone, as shown in Tables Nos. 1 and 2, would, in itself, account for a relatively lower reading of the gauges, thus vitiating any deductions as to bed elevation, unless the corrections were applied, which does not appear to have been done.

The reference to reduction of sectional area below crevasses, and their enlargement after closure as being an invariable experience, appears to be disproved by the records of sections made and tabulated by Humphreys and Abbot,* as well as by other competent authorities, for the special purpose of ascertaining the effects of such outlets.

The general physical characteristics of the stream between the Ohio and the Red River are thus briefly described by a distinguished member of the Commission.

"The bed of the stream is through deposits which it has built up and torn down repeatedly. * * * The caving in the middle third of this portion of the river reaches enormous proportions. A large percentage of the alluvial banks throughout the reach yields readily to the eroding power of the current, and this erosion amounts to an average of about 9 acres per annum for each mile of river. In places the river becomes excessively wide by encroaching on first one bank and then another; * * * again, it becomes exceedingly crooked by the continued erosion of the concave bank.

"The width of the river reaches a maximum in this reach, the high-water banks being sometimes two miles apart. The banks are 30 to 45 ft. in height above low water. Overflows are frequent, except where floods are restrained by levees. The sandbars are very large in extent, and wooded islands and towheads are numerous.

"The extreme range in stage from low to high water is about 53 ft., and the discharge varies from 65 000 cu. ft. per second at low water to 2 000 000 cu. ft. per second at high water (1 : 30).

"The destructive floods enter the alluvial basin at the upper end of this reach and sweep its entire length, gathering strength as they go, and often remain at an overflow stage for a period of nearly three months.

"The elevation of the upper end of the reach at low water is about 270 ft., and at the lower end the elevation is about 2 ft. above sea."†

From the foregoing extract it appears that this reach is characterized by great variation in width, height and volume of discharge, "enormous" caving, long periods of saturation of levees, insufficient depths on crossings, limited to about 5 ft., and frequent overflows and crevasses.

As the levee system is extended, an augmented volume of water is confined to the contracted area, thus increasing the variations of stage and discharge between high and low water and building up the crossing bars. Moreover, the general alignment of the levees as built is such as to cause still greater fluctuations of flow because of the variation in width, ranging from about 2 to over 20 miles, and creating

* See Chapter VI, Hydraulics of the Mississippi River; also, Ellet on the Ohio and Mississippi Rivers.

† "Riparian Lands of the Mississippi River," F. H. Tompkins, New Orleans, 1901.

Mr. Haupt.

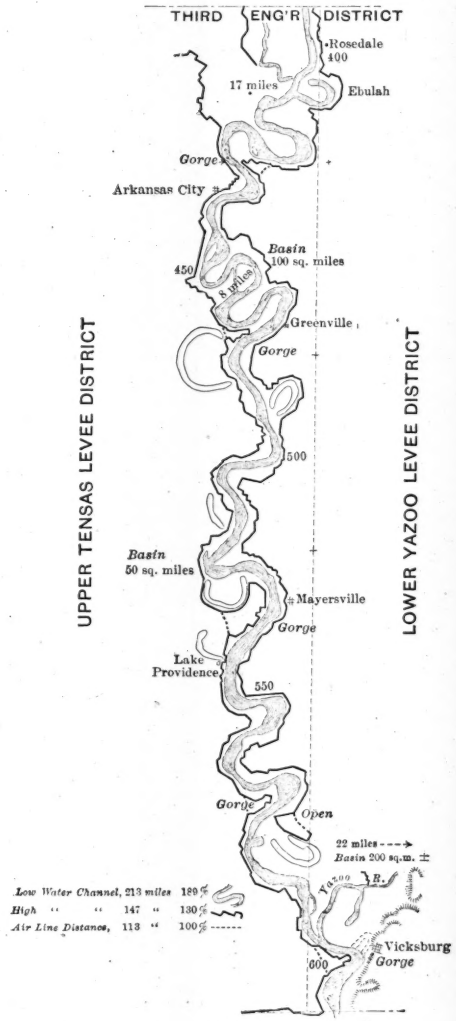

FIG. 4.

Mr. Haupt. a succession of lakes and gorges. The angular trace also opposes greater resistance to the discharge, by reason of the salients and re-entrants, which also add to the length and cost of the works, and increase the caving. In numerous places the construction of a few miles would save from two to three times the length and give a much better flow. These features are illustrated in the sketch of the Third District, Fig. 4.

Evolution of Levees.

The levee is primarily a barrier, erected to defend the plantations from the encroachments of floods. As such, its origin was local, confined to the lower elevations along the river and made to conform to the areas to be protected. These works were built by the earlier occupants of the rich farms, at their own cost, subsequently aided by the Parishes, then by Districts, States and finally by the General Government. The disconnected broken lines were enlarged and connected as the exigencies arose for the continued exclusion of the river from its natural outlet and dumping grounds, until they have assumed magnificent proportions, and yet they are not able to cope with the constantly increasing height of the floods. But the most unfortunate feature of this mode of development has been the absence of any general and systematic alignment for the regulation of the discharge at any stage of the river. The trace of the levees is characterized by a series of broken, angular segments resembling a line of military entrenchments so designed as to enable the adjacent enceinte to be enfiladed. Still more objectionable, however, is the great inequality in width between the lines of levees on opposite banks, or between one line of artificial levees and the natural bluffs, which range from about 2 to more than 20 miles, thus converting the bed into a series of pools or large lakes and chutes. These gorges, being unable to void the floods as rapidly as they enter from above, cause the lakes to fill and overflow, and also produce great irregularities in slope, thus cutting out the bottom at the gorges and filling up the lakes, which at low stages become more difficult of navigation, requiring the use of large hydraulic dredges to reopen the channels, which do not long remain open.

It would apparently afford great relief and a better navigation if the location of the levees were revised and the gorges opened by setting the levees back, which could be done in many instances at a cost which would be fully justified, in the reduction of the total length and in the benefits to both the channel and the farms.

It is suggestive that the cane growers of Louisiana are now advocating the introduction of a system of irrigation to increase the fertility of the lands from which the waters have been excluded by the levees. This must be followed logically by a system of drain-

age and pumpage in places to restore the water to the river, running on the higher level. Fig. 4 will show the defects in the levee trace, as it exists to-day. Mr. Haupt.

EXPERIMENTS TO DETERMINE THE INFLUENCE OF FORMS OF CHANNELS ON THEIR DISCHARGE CAPACITY, AS APPLICABLE TO LARGE RIVERS.

Apparatus.—These experiments are submitted merely in the nature of suggestions, since the apparatus is necessarily so crude as to eliminate all refinements looking to very accurate results, and, even if they were possible, the conditions available would not resemble those in Nature.

As a type, a section of the Mississippi River was taken, covering the Third Engineering District, extending from the White to the Yazoo Rivers, the length being 213 miles by the river or 113 in an air line. Four tubes were taken to represent: (1) the actual length of the stream, following its convolutions at low water; (2) the same developed as a straight line; (3) the river at high water along the axis of the levees; and (4) the same developed.

The two low-water tubes were each 36 ins. long, and had a diameter of 3 mm. The high-water tubes were 23 ins. long, and had a diameter of 11 mm., which gave the straight tubes a relative capacity of 1:13.77. This was checked by filling a given vessel by siphoning through the tubes, under a constant head, which gave the relative times as observed to the nearest second, from a mean of several tests, as 14 : 1.

Experiments.—1. It was found that the receiver was filled by the large tube in 282 seconds, which, being divided by the relative capacity, 13.77, gives 20.44 seconds for the time required to fill the vessel by means of the smaller straight tube. The actual time, as recorded by a watch beating seconds, was 20 seconds, a close confirmation.

2. The low-water straight and bent tubes were then compared in a similar manner by passing a current through them and filling the receiver. It was found that while the time required by the straight tube was 282 seconds that by the bent tube was 1665 seconds, or, in other words, the distortions and bends of the path retarded the flow 5.9 fold.

3. While the large straight tube required 20 seconds to fill the receiver, the corresponding bent tube took 45 seconds, or two and a quarter times as long.

4. The large tubes were then filled and allowed to empty themselves by gravity. The straight one discharged itself in less than one second, while the expanded one required 9 seconds, due to variations of cross-section and interferences caused thereby.

Deductions.—From these experiments it appears that the capacity

Mr. Haupt. of discharge of the convoluted, low-water conduit is but 17% of its rectified length, and that of the distended high-water channel is but 44% of its developed length, thus illustrating clearly the importance of channel rectification to facilitate the rapid discharge of floods.

In a state of nature the river seems to be almost endowed with reason, in its nice adjustment to the demands made upon it, by shortening its length, increasing its slope, opening its mouths, and, if the flood comes on too rapidly, overtopping its banks and depositing its load in the bayous; or, at low stages, falling back into its serpentine bed, with increased length, retarded discharge and a better navigation.

Mr. Thomas. B. F. THOMAS, M. Am. Soc. C. E. (by letter.)—This question can hardly be answered if we are to consider the experience on the Mississippi alone, because the works are not yet complete as to extent, height or section ; but we are at liberty to investigate the effects of such works in other countries, and draw conclusions from their experience of more than twenty centuries, because levees are among the most ancient works which have been executed on watercourses, having been constructed by the Egyptians, Babylonians, Phœnicians, Romans and East Indian nations along the Euphrates, Tiber, Po, Arno, Chiana, Adige, Reno, and other streams.

The question of how best to remedy the evils incident to the succeeding inundations of fertile valleys was discussed by some of the greatest philosophers of the past, among whom were Galileo, Poleni, Torricelli and Zendrini, and this discussion resulted in the establishment of a general levee system. Later came similar systems along the Rhine, Oder, Elbe, Theiss, Vaert, Thames, Mersey, Loire, Vistula, Mississippi, and other rivers of importance. In the ancient discussions, three methods of caring for the excess of flow were considered. These were:

1. To give the promptest discharge possible, so as to pass the waters as fast as they arrive.
2. To retain the flood waters, so as to prolong the duration of the flood, but reduce its intensity.
3. To allow the flood a sufficient space, but circumscribed, in which the discharge will be effected according to its laws, and outside of which the soil (sheltered by levees) will not be subject to inundation.

The first is the clearing process; the second the storage reservoir, and the third the levee system.

Clearing.—Clearing consists of the removal of all obstructions, and straightening and widening the river bed. It does not constitute a remedy, in so far as it affects the principal stream, but, when applied to the side streams in the lower part of a valley, it enables them to discharge their waters more quickly, so that, theoretically, the lower tributaries will have been emptied before the flood arrives from above.

Mr. Thomas.

However, this result is not always accomplished, because the tributaries will not always rise at the time desired, and because, as the sea is approached, the slope is diminished and the velocity becomes less, so that the bed is not capable of discharging the increased flow precipitated upon it, and overflow is bound to result in the lower part of the valley, usually the most fertile and thickly populated. Where this method has been adopted, the conclusion has been reached that:

"It may be a palliation of the evil over some special points or obstacles raising the flood, but not a remedy for inundations in general, and that, sufficiently generalized, it tends to aggravate rather than lessen the effects of floods."

Storage.—Directly opposite to the foregoing are the results aimed at by the method of storing flood waters and allowing them to escape gradually as the flood subsides, or even holding them to assist navigation during low water.

A striking example of the effect of reservoirs on floods is given by Lombardini. The maximum discharge of the Adda, in the flood of September, 1829, at its entrance to Lake Como, was 68 501 cu. ft. per second, while at its exit it was only 28 389 cu. ft. In passing through the lake the maximum flood was diminished in the ratio of 2.4 to 1. This suggests the creation of artificial reservoirs in which to hold the waters during flood times, and this seductive idea has taken strong hold on many engineers of ability, only to be dropped from consideration when its utter impossibility, within reasonable cost, became apparent. Leaving the cost of establishment out of the question, the cost of removing deposits which accumulate constantly, the danger of disaster from breaking when full, a danger far greater than can be expected from extraordinary floods, and the fact that the reservoirs will not always be empty at the opportune moment, ready to catch the waters as they come, must render their adoption unwise and inexpedient, unless other questions of great importance are involved; for instance, if there are groups of interests justifying the establishment of large storage, it might be well to increase the capacity for the purpose of preventing serious overflow.

The writer believes that it is only necessary to study thoroughly the reservoir question, as applicable to the Mississippi problem, to convince anyone that it is impracticable, or even impossible.

Levees.—If one will observe an overflowed valley he will find that over the greater part of its transverse profile a much larger area is being covered than is required for the river's discharge, and that the water is quiet for a great distance on either side of the main river bed. He will at once conceive the idea of confining the water to the space necessary for its flow, and sheltering the adjacent lands from inundation, and the many disasters and inconveniences caused thereby. The embankments by which this is effected have received the name of levees. Their use dates from all antiquity, and has made possible the

Mr. Thomas. building up of rich kingdoms in districts which, otherwise, would have been uninhabitable.

The oldest example of importance, that of the Po, has assured the protection of a vast and rich territory century after century, and still stands as a monument to the skill, perseverance and foresight of Italian engineers, men who dared to act in the face of strenuous opposition and fierce calumny. These embankments follow not only the main stream, but turn up each side of the affluents to points where overflows will not reach, so that:

"The vast submersible plain of the Po is guarded by an immense network of dikes enveloping each stream and defining for all, without exception, a bed in which is concentrated the escape of the floods of the valley."

These levees have been so spaced at and near the mouths of tributaries of importance that the "major bed forms a sort of reservoir in which is stored not only the floods but still, and to disappear with time, the deposits carried by the affluents." In this way the major bed of the main stream and of its affluents in their lower part serves as a regulator for floods, descending from the mountains, which are led out to the sea by a "contracted outlet that tempers the velocity and the disastrous effects by bringing about the raising of the flood above the Panaro." Lombardini has stated, and it has been confirmed in the report of Baumgarten, that this equilibrium has been happily established, so that the discharge is virtually the same at Tessin, Cremona, and toward Ferrara, and that, while the combined discharge of all the affluents amounts to 528 000 cu. ft. per second, the discharge of the Po during the same period is only 176 000 cu. ft. per second. This is certainly a remarkable result, and doubtless much of it must be attributed to the fact that the tributaries do not discharge flood waters simultaneously and that the lakes retard the flow to a marked extent.

The volume of water stored between the levees of the Po and tributaries, from Casale to the sea, is given as 1 896 000 000 cu. m., a quantity corresponding to more than four days' discharge of the river at the rate above given. Thus there has been created an artificial reservoir, the effect of which is the same upon the Po as is that of the lakes upon some of its tributaries, and in this fact lies the secret of their great success. There are, however, very serious drawbacks to levees as a means for preventing inundation, and Belgrand has stated that:

"It is plain that even in a country where levees have existed for twenty centuries, where property has been exposed to all the consequences—the valley of the Po—it has not been clearly demonstrated that the advantages are greater than the inconveniences."

The chief objections to levees are: (1) that they raise flood heights; (2) that they break too easily and often; (3) that they cost too much; (4) that they cause the river bed to rise because they do not permit the escape of sediment over the banks.

Mr. Thomas.

Levees undoubtedly do raise flood levels. Here are the proofs: Some thirty years ago Chief Engineer Dausse made the statement that the level of the highest water of the Po had increased about 2 m. in two centuries, and that while there were only forty-one breaks of levees in the 18th century there were one hundred and nineteen in the first seventy-two years of the 19th, 1872 alone having thirty-six.

On the Theiss, at Szégédin, the following heights have been observed since the levees were built:

1845	21.0 ft.
1855	22.8 "
1867	23.6 "
1876	25.7 "
1888	27.8 "

On the Loire, the coping was originally placed at 15 ft. above low water. After the flood of 1706 it was raised to 21 ft., and in 1846 to 24 ft. Even at that height, the floods of 1856 and 1866 went over in many places, involving most disastrous consequences.

On the Mississippi, the record at Carrollton, La., is as follows:

1828	14.85 ft.
1851	15.05 "
1874	15.35 "
1897	18.72 "
1903	19.05 "

The completion of the system will undoubtedly be the signal for higher flood levels, and the struggle to hold the water within bounds will continue indefinitely. It will not always be possible to prevent overflow, but, in the main, intelligent effort will meet with success, and, while there will always be a feeling of insecurity, yet the conditions will improve with time. A levee is not a perfect barrier against inundation, but it is without doubt the best that can be obtained at reasonable cost.

The second objection noted, namely that levees are unsafe by reason of the ease with which crevasses may be made, is based on conditions which can only be eliminated by care in construction and maintenance at present, and by increase of height in the future, as found advisable.

Levees fail, generally, from insufficient height; sometimes from leakage, sloughing, wave wash, or malicious cutting; but, as a rule, because they have been overtopped by the water.

The objection of cost has no reasonable foundation in fact, because the levees in this country are less expensive by far than those abroad have been, and their final completion and placing in good order does not mean the expenditure of a great amount of money, when all the benefits to be derived from a good and safe system are considered.

Mr. Thomas. There is no other means by which overflowed lands can be reclaimed at such a small expense, and the writer is glad to see that there is a general realization of the importance of the work, and a tendency to more liberal allotments for carrying it on properly.

The charge that the building of levees along a river will cause its bed to rise is contrary to experience and to the laws governing the flow of water, because the very fact that the water is confined in a limited space is an assurance that its velocity will be increased and its transporting power be the greater. Of course, it will have more work to do, because the silt must remain between the levees, but experience proves that the work will be done.

Outlets.—Much has been said and written in regard to carrying off the excess of floods through artificial outlets, but those who have given the subject most thought and those who are responsible for the execution of the projects have long since concluded that the method is not practicable for the Mississippi because there are no advantageous sites, even if the theory is correct, which is an open question. The writer has not noticed this feature discussed in connection with the establishment of foreign systems, so that he thinks it probable that the conditions there are not favorable to its use. The principal advantages and disadvantages of this method have already been given in this discussion (pages 367–371), and nothing further will be added here.

Conclusion.—In 1877 the writer made a journey to Louisiana where he spent several months in the examination of abandoned plantations and timber lands which had been bought, at bankrupt sale after the Civil War, by a gentleman in the North. The principal part of the travel was done on horseback, between the Ouachita and Mississippi. At that time the United States had not undertaken to assist in the construction of the levees, and those built by the State were generally in bad repair and of little benefit.

The writer was daily brought in close contact with the suffering, struggling planters, eking out a mere existence on little patches here and there, while all about them were vast acres of rich, alluvial land devastated by the water. They lived in constant dread of an overflow which might ruin even these meager prospects. There was not a blade of grass or bearing fruit tree for miles. Here and there, mounds were thrown up, upon which the cattle had to stand and fight the flies for days and weeks, without vegetation, except that which was boated to them.

Buildings had "run down" or had been destroyed by the war a few years before. Taxes had "eaten them up" under the reconstruction government. Their case was indeed pitiable in the extreme and apparently hopeless. The writer made one journey of more than twenty miles among plantations, some of which had at one time been

the pride of the South, without seeing a single human face; even the negro, the ever-present, ever-faithful aid to the southern planter, had disappeared from the face of the earth. He met a small drove of hogs swimming toward dry land, one of which, the leader, had almost cut his own throat by striking it with his feet as he paddled himself to safety. Mr. Thomas.

How changed is all this now! Behind hundreds of miles of levees, these people plow and plant, sow and reap, pick cotton, gather fruit, and care for herds of fine cattle, all unmindful of the mighty Mississippi outside. Then they had few neighbors, schools or churches; now these are on every side. The children go to school on foot or drive along good roads, or on the banquette or crown of the levee. All is peace, plenty and happiness where a quarter of a century ago, or less, was despair, almost starvation. If proof were necessary, the changed conditions of the people during this period should be a lasting and sufficient argument in favor of a levee system and of its early completion and permanent preservation by the General Government; and the writer hopes that the good and intelligent people of this country will see to it that public opinion is not led off after strange gods, because a few well-meaning but poorly informed persons are clamoring for something else, their only excuse being that levees do not always and everywhere prevent inundation. Let us as a people hold up the hands of those pioneer engineers, who, although seriously hampered, are working so zealously and intelligently for the general good.

The levee system is not perfect. It never will be as complete as we would desire, but it is far and away ahead of anything else proposed by its opponents, or, within reason, possible, and it has been well advanced toward completion. It would be the greatest folly to abandon a work so well started, or even use it as auxiliary to an untried system, as proposed by some. The thing to be done, as the writer sees it, is to proceed as rapidly as practicable upon the lines laid down, making such modifications as the experience gained thus far seems to indicate as advisable, and when the system as planned has been completed, to maintain it, strengthen it, increase its height, if need be, extend it where it will be advantageous, correct its early errors of location or those parts rendered dangerous by the encroachment of the river, and in every way make it a perfect barrier against flood water, to the end that those who are to be benefited shall realize it as soon as practicable and be thus encouraged to build up and improve the rich territory protected, and thus recover much that has been lost by weary years of patient waiting, uncertainty and deprivation.

Henry B. Richardson, M. Am. Soc. C. E. (by letter).—The form in which the question under discussion is stated seems to imply some disagreement or inconsistency between theory and the results of practice in the matter of levee building on the Mississippi River. If Mr. Richardson.

Mr. Richardson.

there is such thing as a "Levee Theory" for the Mississippi, it has been deduced from the practice and experience of four or five generations of the inhabitants of its alluvial valley who have been constantly engaged in levee building, in reclaiming the wilderness from savagery and preparing it for cultivation and civilization. The answer to the question, if left to these people who have had this actual and immediate experience in "the treatment of the Mississippi River" would be an almost unanimous "yes."

The aim and purpose of levee building, everywhere, is the control of floods; either to prevent destructive overflow, or to fix and preserve the channels through which the water flows at normal stages.

Theory and practice alike agree that the most direct method for confining the waters in times of flood to the path of their ordinary flow is to raise the banks of the watercourses to a higher level, that is, to build levees along streams which are subject to flood and overflow.

A plan which is supposed to contemplate carrying this obvious method to the limit of completely confining all flood waters to the stream in which they naturally gather is presumably what is meant by "the Levee Theory"; while a plan which proposes the regulation of the volume of discharge by impounding the surplus waters before or after they are concentrated in a single stream, or which proposes the diversion of flood waters in excess of a certain stage, or volume, through auxiliary channels or waste weirs, may be another "theory." The levee plan, however, as far as it has gone, has already been the subject of long experience; while the impounding or reservoir plan, and the diversion or outlet plan, are still in the theoretical stage as regards the treatment of the Mississippi River.

The undertaking of works, especially of great public works, cannot be justified, if their plan involves a violation of natural laws, nor if their cost will greatly exceed the value of the benefits expected from them; nor, even, if the benefits in one direction are too largely offset by damages in another. No man, for instance, can justly expect, by any combination of cranks and levers, or realized vision of wheels, to lift himself over the moon by his boot straps, nor justify an investment in the stock of a factory to extract sunbeams from cucumbers; nor prove the economy of cutting off his feet to save shoe leather.

But the control of floods in the Mississippi, as now practiced by means of levees, has been objected to as an attempt to interfere with Nature's laws. Poets and prophets are cited to show the disasters that must follow the prevention of overflows, and the necessity for irrigation in Egypt is instanced as a reason for condemning the application of a levee system to the Mississippi.

It is certain that in a state of nature all the alluvial valley was subject to frequent overflow, and that while thus laid under water it was unfit for the habitation of man, for agriculture, or for most of the

occupations and arts of civilized life. But why it should be a more heinous crime against Nature—even in the eyes of the prophets—to reclaim the wilderness for the uses of man by preventing inundation, than to make the desert fruitful for him by artificial storage and distribution of surplus waters, is not apparent to the uninspired mind. Both of these great undertakings are attempts to modify the course of Nature as it would proceed without the interference of man, and have been carried on in one place or another from time immemorial with great success and benefit. Mr. Richardson.

The fact that the fertility and prosperity of Egypt depend upon overflow or irrigation from the Nile has little application to the condition of the alluvial basins of the Mississippi. The valley of the Nile below the cataracts is generally arid, and is swept by the dry, hot winds of the adjacent deserts. In upper Egypt rain is almost unknown, and in the delta north of Cairo it is rare and scanty. The alluvion of the Mississippi, on the contrary, is bordered by an amply watered and timbered territory, and receives a copious and well-distributed rainfall. Although irrigation is often beneficial, and always necessary for the cultivation of rice, it is readily obtained by gravitation during the high-water season, and by pumping at other times. But agriculture, here, suffers more frequently from the ground being too wet than from being too dry. Drainage and protection from overflow are the greatest needs for the successful cultivation of the soil.

The contention that the bed of the Mississippi is filling up with sedimentary deposits, and thus becoming a less efficient channel for the discharge of its flood waters, and for the needs of navigation, is not borne out by the experience of navigators, nor by the results of the innumerable direct measurements that have been made in the surveys of the river. Those parts of the stream where levees have been longest and most effectively maintained, have the best and most capacious channels. If it were true that the bed of the river is silting up, it would still remain to be shown and explained how and why the levees have had anything to do with it, except as a preventive.

It is said that in its natural condition the flood waters overflowing the river banks deposited their silt on the adjacent lands, greatly to their enrichment and improvement, which is true enough; but the inference that the silt thus deposited on the land would have been thrown down on the bed of the river had the flood waters not been permitted to escape, is not even plausible. From what natural law can it follow that a stream of sediment-bearing water with a volume of, say, 1 000 000 cu. ft. per second should fail to deposit silt in its channel, but that when increased in volume by 50%, or more, it must at once begin to drop its burden all along the road? If the stream were an ox or an ass, one might comprehend it; but, in a river, it seems not contrary to natural laws that the larger the volume—other

Mr. Richardson.

things being the same—the more silt it will be able to carry. The experience of anyone familiar with the Lower Mississippi River should show that when it is in flood it generally carries its heaviest load of silt per unit of volume. When the discharge is greatest, the water is always muddy, and when it is least, the lower river is usually comparatively clear.

But will the cost of a levee system on the Mississippi be a profitable financial investment? If the game is not worth the candle, it has no economic justification. The area of the alluvial valley subject to overflow south of Cape Girardeau is reckoned at 29 790 sq. miles, or, say, more than 19 000 000 acres. Making the very moderate assumption that only one-half of this area will be made to yield agricultural products, when protected from overflow, to the value of only $1 per acre per annum, in excess of what it would have yielded without such protection, there would be gained a yearly revenue sufficient to pay 3% interest, plus 2½% for maintenance, on an investment of, say, $175 000 000; which is nearly ten times the estimated amount now required to complete the levee system. Considering that the land is practically worthless as a source of agricultural profit when not protected from overflow, and that when so protected it ranks among the richest and most fertile in the world, there can be no question as to the profitable results that must follow its protection.

The man who lives and carries on his business in the low lands behind a levee is subject to a kind of danger which does not threaten those who live on the hills, and "they that go down to the sea in ships" incur perils impossible to the man who keeps ashore. But ship building, and ocean travel and commerce, will not cease because a few vessels are foundered or wrecked, nor will levee building be abandoned because of occasional crevasses and overflows; for ship building and levee building appear to be equally justified by experience.

The dual or multiple channel theory of treating the Mississippi River, which proposes to divide it into two or more parallel streams, would increase immensely the length and quantity of levee construction necessary to prevent overflow, because the diverted streams must flow in channels bordered by very much lower land than that near the banks of the main river. Moreover, if the present drainage channels are to be occupied by the diverted flood waters, other channels must be made to drain away the seepage and rain water. The cost of such a system, even if it were possible of application, would apparently prohibit it. The experience with even such leveed outlets as exist, or have existed, although their natural banks are high, has been that larger and larger levees have been required to prevent their overflow; so that Bayou Plaquemines was closed forty years ago, and most practical observers are now convinced that the enormous outlay required

to keep up levees on Bayou Lafourche would better have been expended on the levees of the Mississippi, and that this bayou also should be closed, so that the levee funds now required there may be made available for work on the levees of the main river. Mr. Richardson.

The deficiencies and imperfections of an unfinished structure do not justify condemnation of the theory upon which it is designed. The levee system of the Mississippi is still far from completion, and, like other works in progress, must suffer its share of reverses and accidents, and, for the time being, fail to supply the full measure of benefit expected from it.

There are doubtless many defects in the existing levee systems, some of them hidden or unknown, and others plainly evident; but it is not unreasonable to expect that, in the process of time, the concealed faults and dangers may manifest themselves, and be eliminated as the work of improvement in grade, section and location progresses.

To say that "the river by breaching its ramparts with each successive flood enters its solemn and devastating protest against interference with the designs of Nature," is a figure of speech quite as applicable to the locomotive of a construction train that jumps the rails of an unfinished railway, and should be equally convincing in either case.

Some of the criticisms as to faulty location may possibly arise from imperfect knowledge of the situation. Fig. 4, which shows the alignment of levees for some 200 miles above Vicksburg, may give erroneous ideas to one not familiar with the actual topography. The succession of lakes and gorges that might be inferred from the figure does not exist in fact, as far as the channel of the river is concerned. The excessively wide places between levees are, in almost every case, covered with trees and underbrush, and frequently have levees—not shown in the figure—that prevent flow except along the course bounded by the natural banks of the river. The places where levees on either side are close together are not gorges in the sense of confining the current to materially narrower limits than it occupies in the wider places. The path of the principal current, both at high and low stages, lies within the tract shown by water lines on the figure. An examination of the ground would probably indicate to anyone that the setting back of levees at the so-called gorges would make little difference in the capacity of the channel for carrying or discharging its flood waters.

Whether or not experience with tubes of 3 mm. diameter—or even with those of such large diameter as 11 mm.—can justify a theory for the treatment of a stream like the Mississippi River may be questionable; but it is a question which, perhaps, may best be left for future discussion by the Society.

T. G. Dabney, Esq. (by letter).—The writer, having been invited to contribute to this discussion, will support the affirmative view of Mr. Dabney.

Mr. Dabney. the proposition, and confine his remarks, mainly, to making a partial review of the arguments offered by Professor Haupt.

The first impression made by a perusal of Professor Haupt's discussion is that it has the character of abstract reasoning, rather than the application of practical rules to a practical problem; betraying a lack of the specific kind of information concerning the objects discussed, that can only be acquired by personal and practical contact therewith; which sort of information constitutes a part of the necessary equipment for an intelligent treatment of the subject under consideration.

Thus, a perfect analogy is assumed, by Professor Haupt, to exist in the conditions requiring engineering treatment in the Valley of the Nile, and those that obtain in the Lower Mississippi Valley; whereas no such analogy exists. On the contrary, the Nile Valley, being an almost rainless region, requires the conservation of the annual flood waters, for the purpose of irrigation, as a *sine qua non* to agricultural success; whereas, the Mississippi alluvial plain, having an abundance of rainfall, is burdened with the necessity of excluding the annual overflow from the land, as equally necessary for agricultural success.

Likewise, a similarity of controlling conditions is assumed, between the Yellow River, of China, and the Lower Mississippi; and the unwisdom of leveeing the latter stream is sought to be established by the statement that three millions of Chinese were drowned at one time by the breaking of the Yellow River levees.

The fact that no human life has ever been lost as a direct effect of a breach in the Mississippi levees is sufficient disproof of analogy in these cases.

Professor Haupt favors the establishment of large reservoirs in various parts of the alluvial valley, to arrest and restrain portions of the flood water, and so lower the flood plane. Here, again, is assumed the existence of extensive areas of low depression susceptible of being segregated into reservoir spaces by a simple system of subsidiary levees. But such extensive areas do not exist; and any plan for the establishment of restraining reservoirs must involve the sacrifice of a great extent of valuable property, as well as an exceedingly complex and costly system of interior levee lines, traversing numerous large drainage channels; with such an array of costly and complicated features as to condemn such a scheme as utterly impracticable, in the eyes of those who have a practical knowledge of the situation.

Professor Haupt attaches great value to the use of "outlets" as a means of depleting the flood volume of the Mississippi, and complains that such means of relief have never been tried. Reference is here made to the conditions prevailing in the Atchafalaya River, about 15 or 20 years ago, when a movement was set going, of the rapid enlargement of its channel, that threatened soon to divert the greater part of

the volume of flow from the Mississippi by that route to the Gulf. The Atchafalaya, being one-half the length of the main channel below the point of divergence, a greatly increased volume of flow through it should at once set in motion agencies for flattening the flood slope, or, in other words, of lengthening its channel. This could only be accomplished with requisite celerity by assuming a more serpentine form, through the agency of enormously active attacks upon its banks, resulting in great destruction of property and a chaotic condition of the navigated channel. Mr. Dabney.

These destructive agencies, which had begun to assert themselves at the period named, and created much alarm, were arrested by the timely application of costly revetment and channel sills in the head of the Atchafalaya, and these, since then, have been maintained by constant and careful attention, by the Mississippi River Commission.

How far it may be wise or prudent to set up similar conditions elsewhere, by deliberate design, with all the risks and hazards involved, for the partial and problematical relief to be expected to result, must be left for determination by the body of able experts appointed by the United States for that purpose.

Having touched lightly upon the foregoing heads in Professor Haupt's discussion, their further consideration here is dismissed, as that ground has already been fully covered by other discussors of this topic.

The remainder of this discussion is to be devoted to the discussion of levees, as the more especial object to be treated here.

Professor Haupt objects to the system that has been adopted, of confining the flood water of the Mississippi to its channel by means of levees built along its banks, for the following reasons, as deduced from his discussion.

1.—It is assumed that leveeing the river must result in raising its bed, with the consequent necessity of raising levee grades perpetually to meet perpetually increasing flood elevations.

2.—Increased rate of bank caving in the bends, as a consequence of confining the floods between levees, must involve the danger of crevasses occurring in the levee lines during the prevalence of floods, from breaches due to the engulfment of portions of the levees by caving into the river.

3.—The erosive action of the current is assumed to be greatly increased by the irregularity of cross-section due to varying distances between the opposing lines of levees.

4.—Nature's process of filling up the low grounds in the alluvial plain by deposit from the overflow water, is interrupted by confining all the flood water to the channel.

5.—The danger to the inhabitants and their property, lying under the protection of the levees, is assumed to be increased greatly as a result of higher levees and increased depth of water against them.

Mr. Dabney. Taking up this catalogue of objections to the levee principle in reverse order, Objection 5 is first to be considered.

Professor Haupt enters a sweeping condemnation of the "Levee Theory" in the expression with which his discussion opens:

"This query recurs with each successive high flood, and the river enters its solemn and devastating protest by breaching its ramparts and distributing its surplus where Nature designed it to be deposited."

The inference to be drawn is that the levees have failed to afford any protection to the lands that are dependent upon them. And yet Professor Haupt shows that the levees at Carrollton carried a flood in 1897 that was 4.2 ft. higher than that of 1828. Evidently, that "surplus" was not "distributed," "where Nature designed."

It may be remarked here that those persons who live outside of the so-called Mississippi "delta," and have no personal familiarity with the subject, probably receive very exaggerated impressions as to the magnitude of the disasters resulting from crevasses in the levees, from the overwrought and highly sensational accounts of these situations contained in the newspapers as current news. These accounts are usually made as sensational as the ingenuity of rival reporters can devise, and generally exceed the truth greatly in their portrayal of the evils of overflows.

The great flood of 1897 was heralded abroad in such terms that outsiders were led to believe that the entire alluvial country was inundated. But Major H. B. Richardson, Chief Engineer of the Louisiana State Board, reported that, with his 1 072 miles of levee lines exposed to the attacks of the flood, of the large territory dependent upon them, 85% of the gross area was protected, and a still larger proportion of the cultivated area. Of the 7 000 sq. miles of territory in the Yazoo Basin, two-thirds were protected from overflow by the levees in 1897. Of the thirty-eight or forty crevasses which occurred during that flood, three-fourths of them were in the levees on the St. Francis and White River fronts, where the levees were then in an embryonic stage, and wholly inadequate to afford any protection against a great flood. In the flood of 1903, with only half a dozen crevasses, all told, the measure of protection afforded by the levees was much greater than in any former flood year.

The "three-mile wide" crevasse, which occurred near Greenville, Miss. (in fact, less than 3 900 ft. wide), overflowed less than 10% of the protected area in the Yazoo Basin. The extent of inundated territory, in this case, was increased by the local topography, the escaping water being confined to a trough 10 to 15 miles wide, limited on the eastern side by the high banks of Deer Creek, which extend southward, parallel to the Mississippi, to the foot of the basin.

The fact has been demonstrated repeatedly that generally, with a single crevasse in an extent of levee line of 100 or 200 miles, the

natural channels in the interior of the country are able to carry the escaping water with only local effects of inundated lands. Mr. Dabney.

With increased flood elevations and greater depths against the levees, the volume of escaping water, of course, must be greater, and the local effects proportionably extended. The implied danger to human life may be dismissed as wholly visionary, as no instance has yet come to the knowledge of the writer, in an experience of more than thirty years with levees and overflows, in which a human life was lost as the direct effect of a crevasse.

Objection 4, in the list of Professor Haupt's objections, is to be considered in connection with Objection 1, and Objection 3 will now be taken up.

To this it may be said, the influence of the irregularity of width between opposing levee lines is believed to be unduly magnified, both by Professor Haupt and others who have expressed themselves on that subject.

The volume of water which flows outside of the channel, between the levees is inconsiderable, as compared with the main volume of flow in the channel.

The foreshores, between the levees and river banks, are covered during flood stages by a relatively shallow expanse of water, the flow of which is impeded by the friction of a rough surface encumbered with a growth of grass, weeds, vines, trees, and a variety of obstructions, so that, for the most part, there is little or no movement in this body of water, except in certain localities where the flow is across the neck of a peninsula, from the upper to the lower end of a "bend" in the river, with a shortened distance. In these situations, violent currents are developed, generally near the confining levee lines, the only effect of which is, in some cases, to menace the levee itself by direct erosion, against which special precautions are taken at such points.

There is also some local aberration of the high-water plane, due to this cause, which has to be taken account of in adjusting levee grades. But the suggestion, that the water which is confined by the levees outside of the river channel exerts any influence in accelerating the caving of the banks, seems entirely fanciful and groundless. An ideal location for confining levee lines, would be very near to, and paralleling, the banks of the river throughout all of the sinuosities of the bends, if it were practicable to maintain them in such location, which should prevent the development of local currents and abnormal flood slopes.

Objection 2 attaches importance to accelerated caving as a result of leveeing the river, chiefly because of assumed danger of breaches in the levee line in consequence of such caving.

These breaches have occurred and will continue to occur, but are always foreseen in time to cover the threatened point by a new line of

Mr. Dabney. levee, built in advance of the catastrophe, on a more retired location. The danger suggested by Professor Haupt, that such a breach may occur without warning and at a time when the flood water may pour through it, is wholly imaginary.

The statement that the caving goes on at the average rate of 9 acres per annum for each mile between Cairo and the mouth of Red River, is in error as to the extent of territority to which it applies. The maximum rate of caving is along "the middle third" of the lower river; that is, between the mouth of the Arkansas and the mouth of Red River. Above the former point the caving is much less; and below Red River it is almost nothing. Still, Professor Haupt's argument, if sound, should apply to this "middle third."

It seems to be true that leveeing the river has had the effect of increasing the energy of the caving agencies, a result which should naturally follow from the effort of Nature to enlarge the channel in order to accommodate a larger volume of water. This increased energy, however, is exerted on the bottom as well as the sides of the channel, tending to increase both of its dimensions. But this absolute enlargement of the channel by widening and deepening involves but a moderate excess over the normal extent of caving, and Professor Haupt seems to lose sight of the fact that a displacement of material by caving in one bend only makes room for the deposit of the displaced material from the bend above, which brings this discussion to a consideration of Professor Haupt's objection "in chief," No. 1, to wit, the assumption that the filling up of the river's bed must ensue from the confinement of its flood water.

The more technical treatment of this question has been well covered by other discussors, and this discussion will deal with only the broader, and in some degree speculative, aspects of the question.

It may be postulated that, in any stream, whether it be silt-bearing or other kind, Nature makes an effort to build for it a channel commensurate with the volume of water to be carried; and conversely, in the case of a silt-bearing stream, if the volume of water is lessened to a marked extent, a contraction of its channel capacity should follow. This is exemplified in the Mississippi between Cairo and Helena. In this section, for many years past, perhaps since the seismic convulsion of 1811, about one-third of the flood volume of the river has been diverted from the main river, and carried through lateral channels around the western margin of the St. Francis Basin to a junction with the parent stream near Helena. Here, a case is presented of depleted flood volume followed by a contraction of channel capacity. If a straight line be drawn from low water at Cairo to low water at Helena, it is found to pass from 12 to 14 ft. under the low-water plane at intervening points. Prior to the leveeing of the Upper St. Francis Front, the oscillation of the river at Memphis, from low water to high

water, is recorded as 35½ ft., while at Cairo it was approximately 52 ft., and at Helena approximately 48 ft. Now, when the entire flood volume shall have been confined to the channel by levees, and conducted by Memphis, it may be inferred that, after normal conditions are established, Memphis will have approximately the same oscillation as Cairo, that is, about 52 ft. between low and high water. A superficial reasoner would jump to the conclusion that the flood elevation at Memphis must go at once to 52 ft. on the gauge. But it is demonstrable that the highest gauge reading at Memphis will be ultimately about 10 ft. lower than that at Cairo. So that, to develop at Memphis the same range of flood plane movement as obtains at Cairo, the Memphis low-water plane must be lowered about 10 ft. from the old elevation; and this development will probably ensue in the process of establishing a permanent regimen of the river between Cairo and Helena under the changed conditions to result from concentrating the whole flood volume in the main channel; which can result only by a depression of the general plane of the river bed between those points. Mr. Dabney.

This process, of lowering the low-water plane, has already been observed in operation in other localties along the river, to a less degree, where the demands for such a change were not as great as the conditions at Memphis and points above present.

In the discussion, by Humphreys and Abbot, of the effect of depleting the flood volume by water escaping through a crevasse, it appears to the writer that the data which served as the basis of the discussion were much too meager to justify the weighty and sweeping conclusion arrived at by them.

Their observations were restricted to a single isolated crevasse, the Bonnet Carré; and, in the opinion of the writer, a magnified value is attached to the figures which represent the results of channel and velocity measurements, under conditions, and with appliances, which should demand a considerable allowance as a margin of error. But, it should also be noted that these observations were made in a part of the river where no heavy matter is carried in suspension by the current, but only the fine and light matter, which does not respond promptly to agencies tending to produce precipitation.

If observations of such a character had been made by them in the upper division of the river, where great masses of heavy sand are picked up by the current, carried a few miles and dropped, by a constant and rapidly recurring process during flood stages, it seems certain that the results of such observations would have been quite different.

It is the belief of the writer that a fallacy lies at the bottom of all the speculative theories on the question of the vast capabilities of the Mississippi for building up its alluvial plain by the deposition of sedimentary material.

Mr. Dabney. In considering this subject, it seems to be generally assumed (1) that the great mass of material that occupies the "Mississippi embayment," as it is now found, is of alluvial character, and (2) that the Mississippi of to-day, as it is now known, is the sole agency by which this mass of alluvion was brought there and placed in position.

For the first assumption, it may be stated that it has been exploded by the researches of geologists. Without attempting here to go into the details of these researches, it is sufficient to say that the bed of the Mississippi is not in an alluvial formation of its own creation, but lies below the alluvion, having cut its way down into the underlying tertiary strata, so that it cannot be regarded, from that point of view, as is generally assumed, as being "the architect of its own fortunes." The alluvion, then, is, comparatively speaking, superficial, both in its position and in its quantity.

Again, the Mississippi, as it is now seen, is generally assumed to be the agent by which this alluvion was brought and laid down where it now rests, overlying the tertiary to a depth of some 30 to perhaps more than 100 ft., throughout this alluvial region; and that the same agencies should continue to operate in the same manner, continually building up the alluvial plain at the same rate, and inferentially the river's bed along with it, if not interfered with by the hand of man in preventing the invasion of the interior regions by the overflow water.

These assumptions are believed by the writer to be erroneous. Throughout the alluvial valley of the Mississippi, at least in its upper half, there is generally to be found, a few feet below the surface, very extensive and thick masses of heavy sand, with gravel intermixed, some of the latter being as large as goose eggs. From what is now seen of the river's potency, as a carrier of heavy matter, it seems entirely too feeble to account for the presence of these extensive deposits of such material, which have been conveyed many hundreds of miles in large masses, and laid down in strata many feet thick. The work of transportation of such matter by the Mississippi which is now seen going on, is simply the tearing down of these deposits by the caving of its banks and eroding its bars, carrying it a few miles and dropping it again, lacking sufficient energy to transport it any further. The overflow water escaping over the unleveed banks drops this heavy sediment in the immediate vicinity of the river, carrying none of it into the interior swamps.

Any one familiar with overflows in the alluvial valley must have noted that in the depressions, even a few miles from the river, after they have been filled with river water for a whole flood period, there is no visible evidence of any deposit having been left by the water. The larger trees, which have been standing in the low swamps for centuries, bear no apparent evidence about their roots of any deposit from the overflow water.

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The suggestion is strong, that whatever heavy matter is brought into the river channel at the head of the alluvial basin, or by the tributaries, is a mere local shifting of material, taking the place of other material which has been displaced recently by caving and erosion, to be carried a few miles further down stream. This, of course, should result in a general down-stream progression of this heavy matter, the extent of which movement is to be measured by the invasion of the upper reach of the flat-slope channel in the vicinity of Red River by the shoal-making deposits of sand, and thereby extending the steep-slope characteristic of the river above into the flat-slope division of the channel.

Professor Haupt assumes that with an unleveed river this increment of heavy matter is carried over the banks and into the outlying depressions; and that with a leveed channel it must logically be deposited in the river's bed. The suggestion here offered is, that there is but little increment of heavy matter, of a purely local character, that takes its place in the slow procession of downward shifting bank cavings; and that the true increment, of fine and light matter, is conveyed by the current all the way to the Gulf, and there serves to extend and elongate the channel that the river is really building for itself out into salt water.

The deposit at the mouth of the river contains no heavy silt, but only the finely comminuted particles that are susceptible of being transported by the current to great distances. Thus, in 1877, soon after the jetties in South Pass were completed, an effort was made to extend the lines so as to embrace the newly formed bar outside. The effort was defeated by the exceeding softness and instability of the bar formation; as expressed by the superintendent of the work to the writer, the piles and other materials attempted to be used, "all went out of sight," in the soft mud.

The question now presents itself, by what agency was the alluvion that overlies the alluvial valley transported from the hilly country to the north if not by the present Mississippi River?

The suggestion is, that at a period antedating the birth of the present river, great floods of glacial water, liberated by the melting of the ice cap that reached down a good way south of the region of the Great Lakes, flowed down through the "Mississippi embayment" with vastly greater energy than is now seen displayed by the river, carrying with it the grist from the glacial mills, in the form of sand, gravel, and clay, and laid down the great masses of such matter that are now found there. It is certain that at a time not very remote, geologically speaking, a broad channel, through which now flows the Des Plaines and Illinois Rivers, extended from Lake Michigan down into the Mississippi Valley, through which, as made evident by ancient shore line terraces, now high up on the sides of the valleys, a great

Mr. Dabney. volume of water flowed southward through the Mississippi embayment into the Gulf of Mexico.

It is to be inferred that the present Mississippi River is the degenerate progeny of a much greater parent stream ; and that the vast extent of work of transporting material to fill up the alluvial valley, that is attributed to this progeny, was really the work of its predecessor.

Some idea of the limitation of capacity of the Mississippi for building up its alluvial plain may be had from the progress made in filling up cut-off bends. Three examples may be noted here: Horn Lake, on the boundary between Mississippi and Tennessee ; Flour Lake, opposite the mouth of St. Francis River ; and Moon Lake, about 6 miles below Helena ; all three on the eastern side of the river. Old maps show that these lakes were bends of the river, certainly as late as 1765; but there is evidence that they were converted into lakes by "cut-offs," somewhat more than a century ago.

When such "cut-offs" occur, both ends of the newly formed lake fill up by a rapid process, after which the filling of the body of the lake goes on more slowly. But the conditions to promote a maximum rate of silting up are exceedingly favorable in such situations. In the three cases mentioned a large volume of heavily charged river water flows around through these old channels during every flood period, even at moderate high-water stages, excepting Moon Lake, since the flow has been excluded by levees.

These old channels, lying right in the track of the flood flow, with several miles of their extent at either end occupied by timber and brushwood, would seem to invite the most rapid rate of the silting-up process; and yet a large extent of all three of these lakes is still occupied by open water of considerable depth. Another marked example, which may be included in the same category, is "Old River," into which the Yazoo empties, just above Vicksburg.

To round up this part of the discussion briefly: Professor Haupt's theory is, that a large quantity of heavy silt is brought into the alluvial valley from extraneous sources, and is not carried through to the Gulf. That with an unleveed river this material is carried over the banks of the river by the overflow, and is distributed over the general surface of the alluvial plain. That confining the flood water to the channel by levees must of necessity result in the deposition of this material in the bed of the river.

The opposing view here presented is: That the river is not capable of transporting this heavy silt except for short distances; that the large quantities of detritus washed down from the hills and mountains is nearly all consumed in the process of "base leveling," by which the valleys are raised and the slopes of tributaries are flattened; that but a small quantity of such heavy silt reaches the channel of the

Mississippi River, and takes its place in the slow, progressive down- Mr. Dabney.
stream movement of the material dislodged by erosing from the river banks; that the rate of this movement is to be measured by the extent of the invasion of the head of the delta proper, that is, the flat-slope division of the channel in the vicinity of Red River, by the heavy, shoal-making silt; that, if this movement were as great as is assumed, the entire extent of the flat-slope channel below Red River should be speedily occupied by the heavy sediment carried forward from above, with the result of shoaling its bed, the absence of which effects disproves the assumption; and lastly, the great body of silt material brought into the Lower Mississippi channel consists of finely comminuted particles which are of such lightness as to be conveyed all the way to the Gulf by the river's flow; or that such of it as may be deposited along the way is replaced by other such material gathered by erosion of the sides and bed of the channel.

If Professor Haupt's assumption were correct, to wit, that a large quantity of alluvial matter is carried over the banks of the river by overflow, and distributed over the alluvial plain, then it should follow that the old river bends which have been converted into lakes, and have since constituted the most obvious and convenient catch-basins for sediment, should have been filled up long ago and obliterated from the map as lakes; and that with a leveed river the deposit should be made on the foreshore between the levee and the river bank, and speedily bring it up to the elevation of the levee itself; whereas such deposit is only moderate in quantity.

One other subject is now to be presented, which, perhaps, is not strictly germane to this discussion: Allusion has been made to a constant effort of Nature to establish a state of equilibrium between the three controlling factors in the regimen of the Mississippi, namely, volume of flow, total fall, and length of channel; and that the accomplishment of a fixed relation between these three factors has heretofore been interrupted continually by the recurrence of "cut-offs," at not infrequent intervals in the river's history, as attested by the chain of horseshoe lakes lying along on either side of the channel. The immediate effect of a "cut-off" is to shorten the river channel by the length of the bend thus excised, ranging from 6 to 25 miles.

The accelerated current from steepened slope in the locality of the cut-off, produces greatly increased energy of caving, resulting, generally, in the making of a new "bend," and increasing the length of existing ones, until approximate equilibrium is restored.

No new "cut-off" has occurred during about 20 years past, since the Mississippi River Commission addressed their efforts to preventing such catastrophes by means of bank revetments. But what will be the result if no cut-off ever occurs hereafter? Manifestly, the equilibrium so long futilely sought by the river will be established, by the attain-

Mr. Dabney. ment of a length of channel and a resultant slope, commensurate with the volume of flow, which will satisfy all the requirements of the conditions involved, when further caving of the banks must necessarily cease, as any further caving must increase directly the length of channel, after a maximum length has been reached.

A further result should be, that the current, with flood energy and without a burden of silt to carry, should raze the crests of the shoal bars that extend across the channel at the reversions between bends, carrying the material into the intervening deep pools, and so tend to equalize depths, to increase the discharging capacity of the channel, and, as a consequence, to lower the flood plane; the last-named result to be modified, however, by the flattened slope, to what extent cannot be foretold. But it may be inferred, logically, that, after a fixed and stable regimen is established and maintained among these several contending factors, the Mississippi will adjust its channel to the requirements of its flow, without the necessity of excessive flood elevations.

Mr. Harrod. B. M. HARROD, Past-President, Am. Soc. C. E. (by letter).—The national character and importance of the great work which is the subject of this discussion merits an interest and understanding on the part of civil engineers which it has not generally received, but to which, it is hoped, the present discussion will contribute. In closing, attention must be given to the discussions taking a critical attitude toward the adopted plan, as well as the general features of the river, and the project for its improvement.

Mr. Brown's discussion is of wide range. It covers the physical history of the alluvial valley, the intentions of Nature for its further development, the way in which these have been thwarted by man, the present condition of the valley and river, and a plan for future improvement in which the forces of Nature and the skill of man will combine and co-operate.

The views concerning the past and intended future history of the valley have the quality of originality. Their general discussion may be left to the geologist and physical geographer, but reference to one or two points is opportune here.

Mr. Brown has observed:

"That the river hugs the high country on the east side of the alluvial embayment throughout its whole length, from the mouth of the Ohio to near New Orleans, excepting between Memphis and Vicksburg, and that the tendency of the river throughout is to hug or parallel this higher or older territory on the east."

As the exception mentioned, from Memphis to Vicksburg, where the river is not under or near the eastern bluffs, but wanders across the alluvial plains, is 45% of the length under consideration, or 370 miles out of 830, the "tendency" to hug the high country on the east is not very well defined. But as this higher eastern formation extends to the Gulf, and as the river, by following it, would find a shorter

route to the sea through Lakes Maurepas and Pontchartrain, it appears that, notwithstanding this tendency, the river leaves the eastern and northern limit of the valley for 620 miles out of 1 080, or 57% of its length from Cairo to the Gulf. Mr. Harrod.

The reasons he assigns for this alleged phenomenon are: That the tributaries entering the main trunk from the east have a velocity and abrading force for cutting their channel through the alluvium superior to that exerted by those from the west, due to the greater age, height and abruptness of the eastern hills; that the action of these eastern tributaries is to abrade more rapidly the banks on that side, and, generally, that the western bank has been subjected to less abrasion and wear from its tributaries.

To this it must be answered that exact measurements have shown that erosion is now greater on the western than on the eastern bank; that the superior energy of the tributaries from the east is not, in any way, apparent; that no observations have ever connected the entrance of tributaries and the recession of the banks on the side from which they enter, as cause and effect, and that, if such connection does exist, the result would be a movement of the main trunk toward the west, instead of the east, since all the important tributaries below Cairo, with a single exception, enter the main river from the west, with a volume, energy and duration of flood stage many times greater than that of those from the east. The drainage area on the west extends to the Rockies, while the divide on the east runs through Kentucky, Tennessee and Mississippi, States bordering on the Mississippi River.

The reasons are not apparent why the Lower Mississippi alone, of the rivers of North America, should be charged with breaking Ferrel's law that:

"If a body moves in any direction on the earth's surface, there is a deflecting force arising from the earth's rotation which deflects it to the right in the northern hemisphere, and to the left in the southern hemisphere."

Russell says:

"There is a slight tendency throughout the length of every stream of North America, and at all times, to erode its right bank more rapidly than the left bank."

He illustrates this by the streams on the south side of Long Island.

"Each of the little valleys is bordered on the west or right side by a bluff, while its gentle slope on the left side merges imperceptibly with the general plain. The streams, in each case, follow closely the bluff on the right."

In the case of the Mississippi, Gilbert states that the tendency toward the right bank is nearly 9% greater than toward the left bank, and measurements of caving banks, made by the Mississippi River Commission, confirm this observation. However this may be, the effect must be so slight, and its cause so cosmic and uncontrollable, that it cannot enter, as an important factor, into a plan for the im-

Mr. Harrod. provement of the river. The indirectness of its course, both in the local and minor bends, and in its grand sweeps from side to side of the valley, is accounted for simply and satisfactorily by the fact that the length of the plain is too short for the river. If, at any time, or for any reason, the course of the stream was directly from Cairo to the Gulf, or only about 560 miles long, the slope and velocity could not be maintained, as it would immediately set to work, as alluvial streams always do, to establish meanders, thus increasing its length and reducing its slope and velocity to some sort of an adjustment with the resisting quality of the material through which it flows.

The limit of this discussion forbids the presentation of a plan for the reclamation of the alluvial valley as extensive as that of Mr. Brown, except in its general outlines, and these alone can now be considered.

It provides for an artificial or "secondary stream," between parallel levees, near the hills, and enclosing the lowest lands of each of the great basins. This is to have a discharge capacity of about 600 000 cu. ft., or 40% of a flood volume assumed at 1 500 000 cu. ft. per second. It is to take its source or supply through the right bank of the first western bend of the Mississippi, above Cairo, and, after cutting through several high ridges, and then following the lowest lands of the St. Francis Basin, is to discharge again into the main river, through the present mouth of the St. Francis River, above Helena, or about 315 miles below its source. There will then be a discharge in the Mississippi, at flood stages, at the point of outflow and of return flow, of 1 500 000 cu. ft., and at intermediate points, of 900 000 cu. ft. per second. The "secondary stream" is to recommence on the opposite, left or eastern side, presumably at Yazoo Pass, and, after following along the lowest lands of the Yazoo Basin, redischarge into the Mississippi about 280 miles below, through the mouth of the Yazoo River, above Vicksburg, with differences of discharge similar to those in the first section. The third section begins again on the west side of the river, and, starting nearly opposite Vicksburg, is to join Red River and then seek an outfall in the Gulf along an undescribed route.

On either side of these sections of the "secondary stream" are to be built cross-levees, forming reservoirs to control the discharge; and "suitable waste weirs, of proper proportions, at such points as may be found necessary" to distribute sediment and thus raise the low lands in the interest of agriculture and sanitation.

As this proposed plan for the improvement of the valley and river, by means of a secondary stream, and other supplementary, but extensive works, is in three sections, divided by the main river, an examination of the first, in the St. Francis Basin from Dogtooth Bend to Helena, will suffice, as the two other sections present substantially similar features and equal difficulties and costs.

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Connected at its intake and outlet with the Mississippi, and having a course of nearly the same length as the Mississippi from the Ohio to the St. Francis, the flood surface at start and finish, and at intermediate points, in both streams, main and secondary, in the same latitude, or at proportionate distances from the source, will have the same elevation. What these elevations would ultimately be in the main channel if the flood discharge never exceeded 900 000 cu. ft. per second need not be discussed here. Possibly Mr. Brown assumes that they will always be the same as are now reached by a rising river when the discharge acquires that volume, and that this condition will be permanent. But notwithstanding the violent contradiction of the accepted theory of sedimentary or alluvial rivers involved in the assumption that, if the flood discharge is reduced 40%, the channel capacity will remain undiminished indefinitely, it may now pass unchallenged, for argument's sake.

A flood discharge of 900 000 cu. ft., on a rising river, when passing through the section under consideration, reaches an average bank-full stage, and, were it not for the levees, would be overflowing at many places in large quantities. Trans-alluvial levels show that in the interior of the basin which the proposed plan indicates as the location of the levees, or artificial banks of the new stream, the elevation of the low lands averages about 17 ft. below bank height, or the stage of the main river when the discharge is 900 000 cu. ft. per second. Allowing 3 ft. of free height above this elevation for a safe construction grade, it is found that the new levees of the secondary stream would require a height of about 20 ft., and that the levees now existing on the main river would require no additional grade.

The double line of levees of that part of the secondary stream in the St. Francis Basin, therefore, would have a length of not less than 400 miles, an average height of 20 ft., and contents of 124 000 000 cu. yds. This is more than two-thirds of the contents of the entire levee system, as it now exists, on both banks of the river below Cairo; more than is estimated as necessary for its safe completion; and about ten times as much as will be required for the completion of the levee system of the St. Francis Basin, to which consideration is at present confined. Yet, in arriving at this estimate of one section out of three in the writer's plan, an admission is accepted which hydraulic engineers will generally reject, and no allowance is included for cross-levees, forming reservoirs, "suitable waste weirs at such points as may be found necessary," and for the installation and operation of pumping machinery which will be required when the natural drainage is destroyed.

The natural drainage systems of the great basins lying on either side of the river as it meanders across the valley are well developed, and serve satisfactorily the area now under cultivation, except in the

Mr. Harrod. lower region of Louisiana, where rice and sugar cane are benefited by irrigation and artificial drainage. They, however, are susceptible of great improvement by such ditching and the removal of obstructions from natural watercourses as can be here, and is elsewhere, readily accomplished by corporate, if not by individual, effort. Such progress is now being made in many localities. In this way, not only can the cultivable area be very greatly extended, but the production of each unit can be materially increased. The minor drains are tributary to a main trunk, which, following the lowest levels between the high banks of the river and the hills, gradually assumes the dimensions of a river and finally debouches into the Mississippi, as do the St. Francis and the Yazoo.

It is a part of the plan proposed by Mr. Brown, to build embankments on both sides of this main collecting and discharging conduit, thus cutting off all tributary drainage from its natural outfall, and making it necessary to lift over these embankments, by pumps, the run-off of 50 ins. of rainfall over 20 000 000 acres. In the consideration of this plan, the following quotation is made from Mr. Brown's discussion:

"Our country, to-day, * * * can, perchance, * * * raise the necessary funds to construct and maintain these very large and expensive levee lines" (that is, such as are now two-thirds completed on the river bank); "but in times of adversity, which every country may expect to experience, and funds are lacking to maintain them properly, what will become of this great levee system, which requires constant watching, guarding and the annual expenditure of large sums of money? And if the levee system, when constructed, * * * is by necessity forced to be neglected, what becomes of our rich alluvial country?

"Works affecting such enormous values, as do the works of protection against inundation by the floods of the Mississippi, must, of necessity, to be worthy of adoption, be such as will not confiscate the country in their construction; and also such as will serve reasonably well the purpose for which they were constructed, were they, from expediency, forced to be neglected for a time."

The discussion by Mr. Haupt presents no definite plan as an alternative for the completion of the levee system, but is critical in its attitude, and urges the co-operation of levees, reservoirs and outlets.

The advocates of outlets are careful to state that they do not mean mere gaps in the levees, through which the superabundant flood water may escape, but scientific openings controlled and regulated so as to discharge just water enough. They fail, however, to note that whatever the character of the outlet, or the volume discharged, it must be confined and conducted to the sea, however distant that may be, and not permitted to spread over adjacent territory. This can only be done by more levees, with the additional danger (which is proportionate to the length of line) and cost of construction, guarding and maintenance.

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The alluvial river is divided by many islands. These appear, disappear, and change form and position. A permanent adjustment between the discharge capacity of the main channel and the island chute is seldom, if ever, reached. One is constantly enlarging at the expense of the other. Many islands have joined the nearest shore by the filling of their chutes, while others have crossed the river, which has opened the chutes, by scour, to sufficient dimensions to accommodate the main discharge. The law under which these changes take place will be equally active in outlets. They will either enlarge to uncontrollable dimensions, or silt up and require frequent dredging to preserve their efficiency. Mr. Haupt quotes a description of the ideal channel existing through the 310 miles from Red River down to the Passes, and inferentially assigns this excellent condition to the presence of outlets. With no intention of claiming this as entirely the result of levees, but recognizing more remote hydraulic agencies, it should be stated that but one insignificant outlet exists, or has existed for 40 years, from Red River to the lower end of the levee system, about 275 miles, and to within a short distance of the head of the Passes; that this is now to be closed with a dam and lock because the completion and maintenance of the sub-levee system, by which its discharge is confined and conducted to the sea, is much more costly and difficult than the necessary enlargement of the river levees; and that along this reach of the Mississippi the levees have been built longer and maintained better than on any other part of the river. The "consistency of stage" in this section, attributed by Mr. Haupt to outlets, is really due to the proximity of the outfall, where slope ceases.

With reference to the claim that the opinion that outlets must occasion deposits in the channel falls to the ground after the careful investigations of the sections at certain crevasses, and an extensive series of measurements made with especial reference to testing this assumption, it must be observed that, to have any bearing on the question, such investigations and measurements must consist: First, of a sufficient number of cross-sections to cover some miles of river below the site where a crevasse is to occur, and the repetition of these sections after the crevasse has broken and run for a sufficient time; or, second, of a similar series of cross-sectioning below a crevasse which has run a sufficient time to cause deposit, and its repetition after closure and the passage of a flood of sufficient violence to remove any deposit which may have occurred.

No such examinations have been made, except by the Mississippi River Commission. It is recorded in the physics and hydraulics of the Mississippi River that certain cross-sections were sounded below two crevasses of 1849, and that these soundings were repeated in subsequent years, but the details of the work are not given, and it is not even stated whether, when the later soundings were made, the crevasses

Mr. Harrod. had been closed, and a sufficient flood had passed to show the comparative effect on the channel of an overflowing and confined discharge. Without this information, such observations are of little or no value. On the other hand, the Mississippi River Commission has made surveys at Bonnet Carré, Morganza, at three of the crevasses of 1882, and has continued the comparative surveys of the United States Coast and Geodetic Survey at and below Cubits Gap. All these examinations indicate clearly the reduction of channel capacity as the result of the several outlets, or its enlargement, subsequent to the closure. Similar studies are now being made in connection with the crevasses of 1903.

Tables Nos. 1 and 2, giving the comparison between certain elements of the river, width, area, and mean and maximum depths, in 1881, 1882 and 1883, and in 1894, 1895 and 1896, were presented in the writer's opening discussion as showing a tendency toward greater uniformity of depths and an increase of the discharge capacity of the channel in the thirteen years between these comparative surveys, during which period levee construction had been active. It was then said that:

"There is a limit to the value of the results obtainable even by this exhaustive process, * * * A comparison between the two surveys would be conclusive in proportion to the similarity of the stage conditions preceding them and prevailing while the parties were in the field."

A falling or low stage would have a tendency to scalp or channel the bars and fill the pools, while a rising or high stage would have the opposite effect. The enlargement of width and area at medium and bank-full stages is a permanent result when compared with the elevation and depression of the crests of bars, which vary from year to year from obscurer complicated causes. Crossings which are troublesome to navigators in some years disappear in following seasons as obstructions, to be replaced by others.

In the same part of the discussion other evidence was submitted against the assumption of any general elevation of the bed of the river. Nevertheless, these tables have been cited by Mr. Haupt as containing proof of such a movement. But both the uncertainty of the argument of the tables when applied to the bed of the river and of Mr. Haupt's deductions from them are shown by the fact that the low water of 1895—the year when the later surveys were made—reached a lower stage at every gauge station on the Mississippi, with one exception, from Cairo to Red River, than had ever been reached before, by differences ranging from 0.35 to 1.80 ft. and averaging 1.1 ft. This statement is confined to Red River as a lower limit, because, with the great depth, and absence of bars from there down, the low-water flow line is a function of the volume and friction of the bed, while above Red River the surface is controlled by the elevation of the

bars and crossings, and represents the changes they may be undergoing. This condition of low stage in 1895 appears to have been the result of the depression of the bed, rather than of the small discharge, since in no one of the tributaries, excepting the Wabash, and the Ohio below Louisville, were the lowest stages reached during that year. Mr. Harrod.

Table No. 6, of low-water stages, together with an explanation, is submitted as containing evidence bearing on the changes in the bed of the Mississippi from Cairo, at the junction of the Ohio, to its mouth. Along this part of the river the ten gauges longest established furnish an unbroken record of the daily stages from 1872 to date, a period of thirty-one years. In Table No. 6 is given, for each of these stations, the average low water during the first (1872–1887) and second (1887–1902) halves of the time covered. As the number of years is odd the middle one (1887) is made common to both divisions, in order to include in each the same number of years.

TABLE No. 6.—Low Water, Cairo to Carrollton, 1872–1902.

	1872–1887.	1887–1902.	Difference.
Carrollton	0.1	0.5	+ 0.4
Baton Rouge	3.0	2.7	− 0.3
Red River	3.4	3.2	− 0.2
Natchez	4.6	4.7	+ 0.1
Vicksburg	3.7	0.1	− 3.6
Lake Providence	3.0	1.2	− 1.8
White River	5.5	6.5	+ 1.0
Helena	5.0	2.5	− 2.5
Memphis	2.1	1.7	− 0.4
Cairo	4.3	4.2	− 0.1

It will be observed that the Cairo gauge, which is unaffected by levees, has a substantially unchanged average during the two periods, the difference being only 0.1 ft. From Cairo to the lowest of these gauges, at Carrollton, 100 miles above the Passes, every gauge, with the exception of White River, which has risen 1 ft., and Natchez, which is practically unchanged, shows a lower average of low water during the last half, with differences ranging from 0.2 ft. at Red River to 3.6 ft. at Vicksburg. At Carrollton the depth of the river is so great, and the low stages so subject to tidal and storm influences transmitted up from the Gulf, that the rise there is not connected with changes in the bed, as is the case at the other stations more remote from the sea and where low-water depths at crossings are so small that the profile, at such times, conforms to the profile of the bed.

It may be contended that this reduction of low-water stages in the later years is the result of changes in the physical character of the basin, such as deforesting and improved drainage, which are supposed to cause higher high and lower low waters. This claim could only

Mr. Harrod. be completely met by an analysis of the low-water stages of all the affluents during the period under consideration. These, unfortunately, do not exist, the tributary gauges having been established quite recently, with the exception of the one at Cincinnati. This, however, is the most important, as the discharge from the Ohio controls the stages of the Mississippi below Cairo more largely than any other, and, perhaps, more largely than all the others. But it has been shown, in a table and diagram in the opening of this discussion, that, during the later group of years, the low stages of the Ohio at Cincinnati were higher, and it is here shown that those of the Mississippi were lower, than during the earlier period. This depression of low water below Cairo in later years, therefore, may reasonably be considered as the result of a depression of the bed for that time and on that part of the river.

Mr. Haupt's criticism of the present alignment of the levees is well taken, and will not be objected to by the engineers responsible for it, although the application is to the past practice of levee building, rather than to the theory of the confinement of floods to a single channel by levees. The illustration, however, is unfortunately chosen, as the widening about Mile 400 is the necessary gap between the lower end of the White River and the head of the Tensas Basin, through which the Arkansas and White Rivers discharge, and the levee causing the gorge 30 miles below has been abandoned for several years and retired two or three miles.

The present irregular alignment is only a phase in "the evolution of levees," and is the result of the paramount importance, in the past, for both theoretical and economic reasons, of establishing and maintaining the continuity of the line. When a gap occurred, the only practicable thing, with the means at hand, was to build the shortest possible loop around it. When a narrow and projecting point was reached, the levee was run across it. These imperfections of alignment have long been realized and accepted only as of necessity. It is with levees as with railroads. The first thing is to get there—to open up the country. When the financial development allows more than the maintenance of roadbed and rolling stock, the improvement of grade and alignment receives attention. The rapid influx of population and investment to the alluvial valley has made the extension of protection against such floods as recur with substantial regularity absolutely and immediately necessary, and this has strained the resources of the riparian communities, and the limit to which the Government has, as yet, seen fit to extend its appropriations. With the resources following success and prosperity, the makeshifts of an emergency will be rectified. Even now, the question of alignment is receiving attention, and, in some cases, spurs are built along the axes of points, equalizing the width of the river, and preventing the race of water across them.

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Concerning the disposal by the main river of material derived from the tributaries and its own caving banks, it must be said that an amount nearly, if not quite, equal to that contributed by the caving banks is used in building out and up the opposite shores. This process has been going on ever since the valley was first known, and it has not been observed that it has been hastened by the building of levees. A gradual widening was shown by the earlier reconnaissances and surveys, and in the latest surveys, made within the period of active levee building, and mentioned in the opening of this discussion, the increase of width is surprisingly small. There is now, and always will be, left, on both sides, along the greater part of the length of the stream, or above Red River, about 800 out of its 1 000 miles, a wide foreshore between the levees and the natural bank, for the deposit of material lifted by the stream in flood time. As this is not deeply submerged, and is generally obstructed by a dense growth, the velocity of the overflow is so reduced as to make the conditions favorable for the arrest of suspended sediment.

There is another factor which, although not closely determinable in amount, is evidently of great importance in the disposal of the solid material brought in by the tributaries and transported by the river. The greater fineness of the sand, and the smaller size and quantity of the gravel at lower points in the river is very observable. From Cairo for some distance down there is a gravel bar at the head of most tow-heads and at the entrances of chutes, at points where the depth decreases and the velocity is checked. Passing down the river, the number and extent of these accumulations are reduced, as well as the size of the material of which they are composed. There is no important gravel bed found below Profit Island, 250 miles above the mouth of the river, and this is mainly, if not exclusively, contributed by Thompson's Creek, a small, but torrential tributary from the Mississippi hills. The attrition of particle with particle, on the way down, wears them away, and the result is as if they were dissolved. The fineness of the waste makes it transportable, even at slow velocities, for great distances. It floats from the Missouri for many miles out into the Gulf of Mexico, and has a share in giving the milkiness of color to the water of the lower river with which the settling basin and filter have so much difficulty in dealing. Yet its importance in disposing of solid material is realized from the fact that the loss of half its diameter by a grain of sand or a pebble means the loss of seven-eighths of its contents and weight. It is estimated by physical geographers that 1 400 000 000 cu. ft. of solid material are dissolved annually by rain and other agencies from the surface of the drainage basins, and carried down the main trunk of the Mississippi, in solution. This is not recognized in plans for the improvement of the river and its alluvial valley, and the same is true of the large, but

Mr. Harrod. impalpable, product of the attrition between particles of sand and gravel on their journey from the mountains to the sea.

The dream of using the sediment, carried in suspension by the river and escaping over its banks during overflow, for raising the valley lands, is so attractive that it occurs to everyone who has occasion to approach the study of the physics of the Mississippi Delta, and lasts until investigations show its utter impracticability for general application.

The subject was investigated carefully by Humphreys and Abbot, accomplished students of the Mississippi River, and the ablest advocates of the theory of outlets. The conclusion of General Humphreys, concurred in by General Abbot, is, when applied to the St. Francis Basin, that if the water escaping into this basin during the great flood of 1858 had carried the average amount of sediment for flood stages, it would have sufficed to have filled the basin, if evenly distributed, one-twenty-fourth of an inch, and that the effect in this year was twice that of a year of average overflow. In general, he says:

"The figures exhibited show that such a process on a large scale is impracticable. The only practicable way of reclaiming the swamp lands is to levee the river banks securely, and, as cultivation extends inward, to establish a proper system of drainage."

The most accurate data obtainable concerning the physics of the river show that, as an engineering factor in the reclamation of the alluvial lands, it is absolutely trifling, from its slowness and unequal distribution. The authorities, Humphreys, Abbot, Russell, Davis and others, concur in the estimate that the amount of sediment annually carried in suspension by the main trunk of the Mississippi corresponds to a rate of denudation of the surface of the drainage basin, from this source, of 1 ft. in about 5 376 years. As the area of the basin is about 1 250 000 sq. miles, and that of the land subject to overflow from the Mississippi, 30 000 sq. miles, or a ratio of about 42 to 1, the amount of material from the basin, if spread evenly over the alluvium, would raise it at the rate of 1 ft. in 128 years, or 1 in. in 10⅔ years. But this preliminary assumption is subject to certain limitations. In the first place, high waters of magnitude enough to cause overflow over the natural banks do not occur annually, and great floods are infrequent. Secondly, the period during which overflow lasts, even in floods of magnitude, does not usually last more than two months. Thirdly, the mean quantity escaping during this period cannot be estimated as exceeding one-third of the entire flood discharge of the river. It is submitted, therefore, that an escape of one-third of the flood volume during one-sixth of every year, is a large estimate of the force available for the distribution of sediment over the alluvial lands, and that, therefore, the rate of fill over these lands, if evenly distributed, could not exeed 1 in. in 192 years. Of course, the distribution of sediment would not be uniform, for by far the greater part would be arrested

near the bank, while it is highly improbable that the lowest lands, remote from the river, and needing elevation most, would be raised at a rate exceeding 1 in. in 500 years. Mr. Harrod.

The terms of this problem are so immense and variable that they only admit of its approximate solution, but it is believed that enough has been said to show that, however well it might have been to defer the settlement of the valley for a term of centuries, but little aid can now be expected in its general improvement by sedimentation. The time for this, if it ever existed, has gone by, and the vested interests are too great and active to admit of interruption by any such slow process.

The Mississippi is a sedimentary stream, flowing through an alluvium of its own formation, in a bed adjusted to its needs by its own forces. Although geologically young among rivers, it is mature in the sense of having established a permanent regimen, under which its slope, velocity, length and section are in substantial adjustment with its discharge and the stability of the material through which it flows. It builds up its banks on one side as fast as it tears them down on the other, and strives to maintain its present length.

The phenomena characteristic of this type of river are clearly exhibited throughout its course. With each increment of volume from an affluent, its slope is flattened without loss of mean velocity. Thus, above the junction of the Ohio, it is 0.5 ft. per mile ; from the Ohio to the St. Francis it is 0.424 ft.; from the St. Francis to the White 0.344 ft.; from the White to the Yazoo 0.317 ft; from the Yazoo to the Red 0.265 ft.; from the Red to the Head of the Passes 0.180 ft., and, then, when this combined discharge is subdivided between the several Passes to the Gulf, the slope is again increased to about 0.3 ft. per mile. Throughout this course, in which the slope is steadily decreased as the volume is increased, discharge observations show that substantially the same mean flood velocity prevails, from Cairo to the Gulf, and, therefore, the finer material found in suspension or rolled along the bottom is, toward the lower end, more readily transported.

Wherever, in its course, the flood discharge of the river is decreased, a contraction of waterway by deposit has been observed. This statement applies both to the long reaches along the fronts of basins, where, during unleveed ages, one-third of the discharge of great floods has escaped over the banks, to be collected and returned to the river by the natural drainage system, and also to the outlet through crevasses of magnitude, discharging 200 000, 300 000, and even 400 000 cu. ft. per second.

Surveys, having for their direct object the ascertainment of any changes in the bed of the stream affecting its channel capacity, as the result of natural causes or as the result of levee building, have disclosed a general movement toward uniformity in depth and an increase

Mr. Harrod. of channel capacity. These observations were not confined to a single gauge station or cross-section, but were continued for fifteen years over hundreds of miles of river and thousands of cross-sections.

That part of the subject relating to the elevation of the river bed has been discussed with learning and authority by General C. B. Comstock, M. Am. Soc. C. E., with the following conclusion*:

"The opinion so often held that levees cause a river-bed to rise is probably due to the fact that the bed of a river does sometimes rise although leveed, and hence it is concluded that the levees cause the rise. Any sedimentary stream, having a definite succession of stages and discharges and flowing in its own alluvion, finally takes such a slope as will give a velocity sufficient to enable it to carry its sediment, whether derived from above or from its own banks and bed, farther down-stream, without, on the whole, scouring or filling its bed. An average velocity less than this will give rise to deposits in its bed, or if it is crooked it will become straight, thus in either case increasing its slope and velocity towards their normal values. An average velocity greater than this will scour its bed, or cause caving in its concave bends, thus increasing its length and diminishing its slope and velocity to such values as its bed can bear without on the whole scouring or filling. When therefore the slope of a sedimentary stream suddenly diminishes from that which it needs for a stable regimen its velocity also diminishes; it drops part of its alluvion, and its bed rises. Thus, when the Mississippi enters the Gulf of Mexico, its slope suddenly diminishes; its velocity diminishes, and it builds up bars out in deep water. So the Adige, when it reaches the low plains of the Po, needs for permanence a steeper slope than the country has, and raises its bed above it. In all these cases the bed would rise without levees."

"There is one more cause for the rise of bed of a sedimentary river, which, however, acts at a very slow rate. The Mississippi pushes its mouths out into the Gulf at the rate of about 4 miles in a century, and this increase in length requires a corresponding increase in fall of water surface to make the waters flow out. An increase of 4 miles in length would, with existing slopes, raise the high-water surface at New Orleans about 0.7 ft. The cost of raising levees to correspond with this rise in the water surface would, as has already been stated, be a small part of the annual cost of the system."

That the confinement of floods between levees would have the immediate effect of increasing flood heights was, of course, anticipated. But it can be said, even on the assumption of a permanent or inerosible bed, that this increase of height will only be in proportion to the increase of discharge, and that to this there is a limit. There have been facts given in the opening of this discussion which lead to the conclusion that this limit, as far as the discharge of the affluents is concerned, has been nearly, if not quite, reached, and that there only remains the improbable contingency of a more synchronous combination of tributary discharges than has ever yet been known. But the bed is not inerosible, and there are reasons given in the preliminary discussion why the forces of a uniform and combined flood volume will, in time, remove the accumulation of sediment left in the bed by high

* Appendix A, Report of the Mississippi River Commission for 1890.

waters depleted along the basin fronts through ages of overflow, and thus abate the increased flood heights now experienced as the immediate result of levee building. Mr. Harrod.

Reservoirs and outlets, like levees, must be judged both by their conformity to hydraulic theory and by their practicability. Withholding the excessive part of the flood discharge of the tributary by reservoirs is distinctly an aid in abating flood heights in the lower river, and thus in the reclaiming of its low lands from inundation, and the same means serve for irrigation where most needed. Thus the irrigation of the arid lands and the leveeing of the alluvial lands are closely connected as co-operative projects. Unfortunately, the practicable extension of the reservoir system is very limited. It finds no application on the Ohio, and very little, as a factor in reducing floods on the lower river, on the Upper Mississippi.

Below Cairo it is quite impracticable. The only basin which has been proposed as a reservoir site—the St. Francis—comprises 6 000 sq. miles of land of great fertility and increasing value, the homes and improved properties of a large population, and hundreds of miles of railroads, not to mention two Congressional Districts. Besides the acquisition of the necessary rights, the works for impounding and controlling the surplus flood waters would be very extensive and costly, requiring skillful and probably dangerous maintenance and operation. Theoretical questions concerning the permanence of channel capacity and of continued flood relief with a reduced discharge also arise as objections to such a project. At this late period, when the basin is so largely occupied and advancing in wealth so rapidly, and when the levee system for its protection is so far advanced, the proposition does not present either economic or engineering advantages. The present and prospective value of the site for farming and industrial purposes is enormous, and its protection from overflow by levees can be accomplished more cheaply, more safely and at an earlier time, than by any other method.

The theoretical objections to outlets are well stated* by General Comstock as follows:

"A sedimentary river flowing in its own alluvion only acquires a stable regimen when it has taken a slope suitable to its varying discharges and to the material through which it flows; and, as a rule, these slopes diminish as the size of the river increases, and increase as the size of the river decreases."

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"These examples are sufficient to illustrate the general rule already stated, that sedimentary rivers flowing in their own alluvion take larger slopes the smaller they are. Hence, if at Lake Borgne or elsewhere in its Delta, the Mississippi were divided into two rivers, since each would be smaller than the present river, the two new rivers would go to work to obtain the new and steeper slopes suited to dimensions

* Appendix B, Report of the Mississippi River Commission for 1890.

Mr. Harrod. smaller than those of the original river, and hence would build up their beds. This process would only cease when the steeper slopes needed by each were obtained. Since both rivers would then have one end at the gulf, and have steeper slopes up to their point of divergence than the main river now has, the flood surface of the river at that point would be higher than now."

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"It may be concluded that the reduction, by any large amount, of the flow of the Mississippi at Lake Borgne will be ultimately followed by a rise in the flood heights at that place and a shoaling of the river below and at its mouth."

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"Both theory and experience show that when, at all stages, a reduction in the size of a river flowing in alluvial soil is made, or the river is split in two, the smaller rivers gradually take greater slopes than the main river had. Hence both the main river and the new river would gradually increase their slopes to suit the new conditions. Since the slope begins at the Gulf, it can not become greater on the main river below Lake Borgne, which is now nearly straight, without increasing flood heights at Lake Borgne. After some years, then, if both routes to the sea remained large rivers, the flood level above the outlet would be higher than it is now, unless, as indeed is not improbable, the large amount of sediment which would be dropped into Lake Borgne (where the flood velocities would at first be but about one-sixth of those in the Mississippi) should close this outlet, thus repairing the injury done to the main river. A large diversion of flow from the Mississippi to Lake Borgne would also seriously diminish the depths at the present mouths of the river."

When to these objections are added the practical difficulties and costs of controlling the discharge of an outlet with additional levees and regulation works, the conclusion is inevitable that the reclamation of the alluvial valley from overflow can be accomplished most cheaply, safely and speedily by the completion of the levee system to sufficient grades and dimensions.

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TRANSACTIONS.

Paper No. 966.

SEWAGE PURIFICATION.

An Informal Discussion at the Annual Convention, June 10th, 1903.

By Messrs. Rudolph Hering, George W. Rafter and L. J. Le Conte.

Rudolph Hering, M. Am. Soc. C. E. (by letter).—The writer's compulsory absence prevents him from opening this discussion in person, as he had intended, namely, to outline the entire subject in its essential parts, dwelling upon those methods which are now more or less on trial, and concerning which there is a lack of enough experience to eliminate all differences of opinion. Instead, he sends the following brief remarks: Mr. Hering.

This subject stands to-day, in a totally different light from what it did less than twenty-five years ago. A complete purification of sewage now resolves itself into the necessity of establishing the best conditions for the most suitable bacterial life to become most effective, because only in recent years have we recognized this minute life to be the essential feature in the process.

Bacteria may be active in flowing, or in large bodies of comparatively quiet, water. To get the desired effect, the bacteria contained in the sewage itself, its proper dilution, accomplished in an inoffensive manner, is therefore one of the important questions, and it is not yet completely answered.

The purifying bacteria may also be active in porous soil, notably in sand, and in imitation of Nature's production of spring water, the result may be completely satisfactory under suitable conditions, which are now fairly well understood.

Under still other conditions, a different class of bacteria, also contained in sewage, is brought into service, or the same bacteria may

Mr. Hering. be brought to act under modified conditions, and, with the aid of the liquefying process of putrefaction caused by them, we may get rid of much of the suspended matter, which is the most troublesome element in all phases of sewage purification. But such aid only partially solves the problem. Bacteria must again be set to work to convert the liquid sewage finally into mineral and inoffensive solutions. In order to gain this end, a number of practical ways of accomplishing these different processes are now in use.

Numerous local conditions have been the cause of a variety of solutions, some of which are still on trial and await further investigations and study from actual experience on a large scale. In America and in Europe this experience is now being gathered, and a general discussion of the recent results gained should be instructive and valuable.

Mr. Rafter. GEORGE W. RAFTER, M. Am. Soc. C. E. (by letter).—Sewage disposal has always been a simple enough matter to the non-technical people who mostly control the affairs of municipalities, but to those charged with its administration, it has been, and still continues to be, exceedingly complex. The latter class are conservative in estimates and statements, while the former are positive that they, at any rate, have struck the final solution.

When the matter is examined historically, it is found that sewage disposal, pendulum-like, has swung back and forth between certain extremes. From fifty to sixty years ago the sewage nuisance had so far forced itself upon public attention that a considerable number of works were constructed in England, and in the report of Henry Austin, "On the Means of Deodorizing and Utilizing the Sewage of Towns," published in 1857, a general *résumé* of the subject is given. At that time, chemical precipitation was coming into use, and extravagant notions of the value of the manure to be obtained were indulged in. It is unnecessary to state that none of these methods has justified the hopes of its inventors. A great many patents were taken out for various chemical processes, from about 1835 to 1860 or 1870, but the majority of these patents were so impractical as never to go into operation. Indeed, sewage disposal, owing perhaps to its complex nature, is not properly subject to patents. The useful developments, thus far, have been mostly the work of scientific men, and ordinary inventors have scarcely contributed anything thereto, nor are they likely to, because the complexity of the operations is such as to be beyond them.

The separation of the solid matters in suspension by sedimentation, straining or filtration, however, is an interesting process, which was tried in England about the year 1850, and which, from its developing on lines quite similar to what is now known as the septic process, may be considered a little at length. Mr. Austin describes the works at

Cheltenham, England, for what he calls a mechanical process for separating the solid matter of sewage, as follows: Mr. Rafter.

"The building is divided longitudinally, forming below ground two sets of reservoirs or tanks, which are employed alternately. The sewage passes through vertical filters in the upper and lower tanks, whereby the great bulk of the matters in suspension is separated and retained. These filters are 5 ft. deep and 2 ft. thick, and consist of coarse gravel inclosed within 2-in. perforated boards, these being protected with basket work to prevent clogging.

"The heavier matters of the sewage deposit themselves at the bottom of the tanks, but a large proportion of the solid forms itself into a floating body, and accumulates to about 18 ins. thick on the surface. The liquid is conveyed from the angular filters in the upper tanks by a line of pipes in each division.

"A weir, or rather division, in the third or lining tank, causes the water, then partially clear, to flow through a channel at each end, during its passage through which a certain proportion of cream of lime, mixed on the floor above, falls into it, and occasions a further precipitation to take place. The effluent water then passes through another filter with gravel finer than before, and then through a third, finer still, to the outfall.

"When either reservoir contains a certain amount of solid matter, the flow of sewage is cut off and turned into the other. This takes place about every eight weeks, and the filtering medium of gravel is removed at the same time and washed. The contents of the tanks which are in a state of 'slush' are then hoisted in buckets through the trap lids on to the floor above, and wheeled out and mixed with the scavenger's refuse of the town, the ashes, street sweepings, etc. These are brought to the yard, and a kind of embanked reservoir is formed of them immediately outside the building, and as the semi-solid sewage is wheeled into the midst, the dry refuse outside is turned on to it. The liquid is at once absorbed, and after being turned over and thoroughly intermixed, the solid mass is fit for immediate removal and use.

"The ashes and dry refuse of the town are said to be just sufficient for the purpose. They absorb about two-thirds of their bulk of sewage; that is to say, one cubic yard of the ashes, etc., and two-thirds of a cubic yard of the semi-fluid sewage, make only one cubic yard of solid manure."

The foregoing quotation is especially interesting as indicating that Mr. Austin observed an operation in the Cheltenham tanks exactly as is now known to exist in a septic tank; that is, the heavier matters deposit themselves at the bottom, while a considerable portion of the solids forms itself into a floating body, accumulating to the thickness of about 18 ins., on the surface. In view of this fact, it seems fair to say that in a practical way Mr. Austin was in reality the discoverer of the septic tank. Nevertheless, the real significance of the septic tank —the fact that the changes going on are biological—was not appreciated by him, as is shown by his calling it a "mechanical process for separating the solid matter of sewage." Nor, indeed, does anyone seem to have appreciated the significance of these tanks, although a number of them were built and operated in England, with such success as is pos-

Mr. Rafter. sible for that kind of a tank, but the so-called chemical purification process forged rapidly to the front, and mechanical separation—which is now called septic action—was lost sight of for a number of years, and chemical purification became for a time the leading form in England.

In the meantime, land purification processes, where the proper conditions obtained, were slowly gaining ground. These processes have always labored under this disadvantage, that not being in any way subject to patent, they have never had the booming which chemical precipitation and purification by septic action have received, although whether this is so great an advantage may, perhaps, be an open question. At any rate, the progress which such processes have made, while slow, has nevertheless been sure, until we are justified in saying that where proper conditions obtain, broad irrigation and intermittent filtration are, all things considered, the most satisfactory systems yet worked out. As a broad proposition, in consideration of the value of the crops produced, broad irrigation is the cheapest system yet devised. Experiments at Berlin, Paris, Croyden, Doncaster, Birmingham and other places, show that large crops may be produced, and eaten without prejudice to health.

Let us now consider the recent development of sewage purification by means of bacterial action. This has been a favorite subject for consideration by a number of biologists, scientific writers, etc., for a good many years. The history of the matter will not be gone into at length at this time, any further than to point out two or three significant guide posts along the line. Like all other discoveries, its development has been gradual, and is the work of many men instead of one.

As far as can be learned, Dr. Alexander Mueller was the first person to appreciate clearly that bacteria were the prime agents in sewage purification. About 1880, Dr. Mueller took out a patent, endeavoring to utilize the micro-organisms in sewage for purposes of purification. Works were built under this patent, and were in operation for some time, to purify the effluent of a manufacturing establishment for beet sugar. Dr. Mueller says:

"The contents of sewage are chiefly of organic origin, and in consequence of this, an active process of decomposition takes place in sewage, through which the organic matters are gradually *dissolved* into mineral matters, or, in short, are mineralized, and thus become fit to serve as food for plants. To the superficial observer this process appears to be a chemical self-reduction; in reality, however, it is chiefly a process of digestion, in which the various, mostly microscopically small, animal and vegetable organisms utilize the organically fixed power for their life purposes.

"The decomposition of sewage in its various stages is characterized by the appearance of enormous numbers of spirilla, then of vibrios (swarming spores), and, finally, of moulds. At this stage commences the reformation of organic substance, with the appearance of the chlorophyl-holding protococcus, etc."

Mr. Rafter.

In the meantime, in December, 1881, and January, 1882, an account of Mouras' automatic scavenger was published in France. This apparatus consists of a closed vault with a water-seal, which it is stated rapidly transformed excrementitious matters into a homogeneous fluid, only slightly turbid, and holding the solid matters in suspension in the form of scarcely visible filaments. The principle upon which the automatic scavenger acts is that animal dejecta contain within themselves all the principles of fermentation necessary to liquify them and to render them useful. Later observations show that Mouras' view, that animal dejecta contain within themselves the fermentative principle, is fundamentally true, and, in this particular, Mouras, with Dr. Mueller, was in advance of the scientific workers of that day.

A number of investigators were at work upon the general problem, and in May, 1886, Dr. Dupré, in discussing a paper of Dr. Tidy's, at the Society of Arts of England, proposed to cultivate bacteria on a large scale, discharging them with an effluent into a river where they might be expected to further purify sewage.

In January, 1887, Mr. Dibdin read a paper, before the Institution of Civil Engineers, in which he discussed the propriety of using large quantities of lime. At that time many chemists claimed that lime was especially valuable in sewage purification because it destroyed living organisms, such as bacteria, which give rise to putrefaction, the idea being that, if all the organisms could be destroyed, sewage would be rendered innocuous. Mr. Dibdin took the ground that the very essence of the sewage purification is not the destruction of bacterial life, but is the resolution of organic matter into other combinations. Hence, it follows that an antiseptic process is the very reverse of the object to be aimed at. That is to say, a septic process is required, and hence the name septic sewerage applied later on to designate this process.

In November, 1887, the Massachusetts State Board of Health commenced their classical experiments on the purification of water and sewage by filters. The first two volumes of the reports, published in 1890, laid the foundations of the modern development of bacterial sewage purification processes. The experiments were carried out with an unparalleled wealth of detail, and, taken all in all, they are still unapproached by work done anywhere. The reports have been continued from year to year down to the present time. As far as known to the writer, no criticism of the work of the Massachusetts State Board of Health has ever been made, except that their experiments were on somewhat too limited a scale to be entirely comparable with practice, nevertheless, the general deductions have been confirmed at a very large number of sewerage works, both in Europe and America.

Mr. Rafter. An English engineer, W. D. Scott Moncrieff, constructed experimental works for the bacterial purification of sewage in 1891. He constructed a bacterial tank, in which the old principle of upward filtration was applied, the crude sewage being admitted from below and gradually flowing upward through a bed of stones. In this way he found that the liquefaction of solids was so effective that the sludge of ten persons was absorbed on 9 sq. yds. of land. The space beneath had a capacity of less than 5 cu. ft., and would obviously have filled up except for the liquefying action which had taken place.

In 1892, Mr. Dibdin began his experiments on filtration through coarse material, such as burnt ballast, coke breeze, gravel, etc., the object being to obtain a high degree of porosity, with power of reabsorbing atmospheric oxygen. In the first series of experiments made, wooden tanks, with an area of $\frac{1}{200}$ of an acre, were used.

In the second series, the filter covered an acre of land, the material consisting of 3 ft. of pan breeze, with 3 ins. of gravel. These experiments were continued from September, 1892, to November, 1896. Space will not be taken to indicate the results, any further than to say that, generally, the bacterial purification of sewage was advanced considerably thereby.

In the meantime, Mr. Dibdin had also conducted experiments for the Sutton Urban District Council, and, in 1896, constructed a bacteria tank for that authority. This tank was fairly satisfactory, and, in 1897, the District Council gave orders for a second tank to be turned into a similar bacteria bed for treating crude sewage. The first tank was filled with raw sewage entirely untreated except that it had been strained through a screen to intercept the grosser particles. During seventy-six days, sewage was treated at the rate of 773 000 galls. per acre per day, rest period included. In the London experiments, 1 000 000 galls. of the effluent from chemical precipitation were treated on the burnt ballast bed of one acre, as against about 60 000 galls. per acre per day treated through coarse sand, as per the system of the Massachusetts State Board of Health.

About 1897, Mr. Cameron designed and placed in operation at Exeter, England, a septic tank. This tank is the septic tank as we now understand it. Its features are the reception of sewage, without any preliminary treatment, in a covered tank having a capacity equal to anywhere from 12 to 24 hours' flow of sewage. The cross-sectional area of this tank is large enough to permit the sewage passing very slowly through it, thereby depositing the heavy matters, leaving only the lighter ones to pass forward with the water to the coke breeze filter beds, constructed on the same plan as in the London experiments. A comparison of the plans of Mr. Cameron's tanks shows that in the essentials it is similar to the tank in use at Cheltenham, England, and also the experimental tank proposed by Mr. Austin in

his report of 1857. To appreciate this fact, one needs to have Mr. Austin's report before him, together with plans of a Cameron tank. Mr. Rafter.

The septic tank has been experimented upon by the Massachusetts State Board of Health, and a large amount of valuable matter is given in the reports of 1898–1901, inclusive. These experiments, however, are on somewhat too small a scale to be decisive, although probably the main conclusions will be verified by more extended experiments. The most satisfactory experiments carried out in the United States have been made at Worcester, Mass.

As to the success attained in the purification of septic sewage, as a whole, the results are somewhat contradictory, although, under certain conditions, the process may be a material addition to a sewage purification plant, but success cannot be depended upon in every case. Apparently, a distinction must be made between fresh, stale and septic sewage, and sewage mixed with refuse manufacturing waste, such as refuse from tanneries, slaughter-houses, acid-works, cheese factories, etc. All these different forms of pollution need to be experimented upon before a final general rule can be formulated.

As regards the treatment of fresh sewage, the results indicate that an accumulation of sludge within the septic tank must be expected, and amounts to a considerable percentage of that originally present in the sewage on entering the tank. Enthusiastic advocates of bacterial purification have affirmed an entire doing away with the sludge difficulty, but, thus far, this claim has not been verified. The sludge difficulty still remains, and when sewage works are operated on a large scale, will be the source of considerable trouble in the future, as it has been in the past.

With stale sewage, which has undergone mechanical, chemical and bacterial action while flowing through the sewers, it is possible that the action in the tank will be much more satisfactory than in the previous case. Experiments indicate that a considerable proportion of the organic matter in suspension will be removed either by liquefaction or by changing to a gaseous form, and that its effluent can easily be further purified by filtration.

In the case of septic sewage, which has already been subjected to bacterial action, further bacterial processes may not only be unnecessary, but even harmful, because they render sewage difficult to purify by subsequent oxidation.

As to the effect on sewage containing manufacturing wastes, the report of Messrs. Kinnicutt and Eddy, "The Action of the Septic Tank on Acid Iron Sewage," is perhaps the most authoritative of any experiments thus far instituted. These gentlemen state that from the slow passage of an acid iron sewage containing an amount of organic matter, represented by about one part of albuminoid ammonia in one hundred thousand parts, an amount of iron in solu-

Mr. Rafter. tion from five to eight parts and the free acid in terms of sulphuric acid about ten parts in one hundred thousand parts, the following results may be expected:

"(1) That about one-fourth of the total solid matter contained in the sewage will be removed.

"(2) That the effluent from the tank will contain about 20% less soluble matter than the crude sewage, owing to the soluble matter in the sewage being decomposed or changed into insoluble substances.

"(3) That the amount of suspended matter removed from the sewage will not greatly exceed 30% unless special precautions are taken to retain in the tank the finely divided iron sulphide, formed by the reduction of the soluble iron sulphate in the sewage.

"(4) That the amount of organic matter removed from the sewage, as determined from the albuminoid ammonia, will average from 20 to 25 per cent.

"(5) That, of the suspended matter, the amount removed will average a little under 50%, and that the amount of soluble organic matter removed will not average much more than 10% of the soluble organic matter in the sewage."

It is also stated that while the sludge from such a tank will be proportionately from 60 to 70% of the original, nevertheless, not more than 50% will require to be removed, owing to the fact that the sludge in a septic tank contains less water than in a sedimentation tank.

The definite results gained by the Massachusetts State Board of Health and at Worcester show that the purification effected by a septic tank is so slight as to be of very little advantage, and the septic tank, alone, cannot be considered a means of sewage purification. It must be considered a method of transformation, merely.

In conclusion, the writer's view is that while the septic tank, and purification by bacteria generally, has considerably advanced our knowledge of sewage purification, nevertheless, as a method, it will, in the end, take its place with mechanical separation, chemical purification and many others, which have been temporary fads only, but that land purification processes, where there is suitable land available, have the advantage of not only effective purification, but, by reason of the value of the crops, make possible a partial or complete return on the outlay. Where suitable land is not available, less efficient processes are permissible.

Mr. Le Conte. L. J. Le Conte, M. Am. Soc. C. E. (by letter) —Sewage purification is rapidly becoming a matter of grave public importance, and calls for a high type of intelligent supervision in order to insure good results.

In the younger States of the Union, the population being sparse, the question of stream pollution does not arise, but as the population increases and manufacturing establishments begin to develop, the pollution troubles begin to show themselves, and endless complaints are duly filed, and end in the Courts with suits for damages sustained.

Mr. Le Conte.

The oldest and most densely populated State in the Union is Massachusetts, and there the troubles regarding stream pollution are most pronounced. Hence, when troubles of this nature arise in a younger State, one turns naturally to the old Bay State for advice and counsel as to the future requirements which are likely to be enforced by the Courts.

The State Board of Health of Massachusetts has been and is now spending large sums of money in quietly experimenting—on a commendable scale—to find the true value of the different methods of sewage purification. These results are published in their annual reports, which really constitute some of the very best information available on the subject.

In a general way, it may be said that when a natural stream becomes highly polluted at low stages, so as to become an intolerable public nuisance, then some palliative action must be taken to remedy the evil. Hundreds of devices have been tried, but with indifferent success. Probably the most practical methods are:

First.—By percolation through porous soils, or sand—being, in short, broad irrigation—the purified filtrate passing into the stream. This method, necessarily, calls for a large tract of land of suitable character, which is seldom available.

Second.—By preliminary treatment, such as settling, septic action, chemical precipitation and filtration through coarse material, in order to reduce the quantity of suspended matter as well as the dissolved organic matter, both being preparatory to a final treatment, either by dilution or by intermittent filtration through porous soils. By this last method, the acreage required is very much reduced.

After the sewage has passed through tanks providing subsidence and septic action, it may be turned into the stream, always bearing in mind that the sewage from one thousand people may be safely diluted by a river flow of 4.0 cu. ft. per second. Such disposal will be quite inoffensive when discharged in the middle of the stream. The remainder of the sewage, over and above this amount, must go to the intermittent filter beds, the purified filtrate from which may also be discharged into the river almost anywhere. If this is done properly, and the details are attended to faithfully, the results will be highly satisfactory.

In cases where suitable land is not available, it may become necessary to construct the entire filter beds artificially, which will add very materially to the first cost and to the maintenance.

Of course, each particular case calls for a special study in itself, in order to get the best results with the least expense. This, after all, is the main result to be attained, and, in order to be reasonably sure of obtaining it, a competent expert should be engaged to study the case.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

Paper No. 967.

SOME RAILWAY CONSTRUCTION IN OKLAHOMA.*

By A. G. Allan, Assoc. M. Am. Soc. C. E.

With Discussion by Messrs. Samuel H. Lea, Emile Low, H. F. Dunham, J. P. Snow and A. G. Allan.

During the past eighteen months the writer has been in charge of about 60 miles of new railway construction, on which were built a number of different structures, some of them not usual in new railway work in the West, and, thinking that perhaps it might enable someone similarly situated to make approximate estimates of cost, or help in the design of masonry to be built in advance of the track, he begs to submit this paper.

It would be proper to state that there is no claim for originality in the plans, many of them being merely modifications of the standard plans of the road, the others having been used elsewhere by other members of the engineering profession.

All the construction described is on a portion of a line being built by the Atchison, Topeka and Santa Fé Railway from Newkirk, Oklahoma (a point 14 miles south of the Kansas State line), to Pauls Valley, Indian Territory.

As it is intended to use the new line for heavy freight traffic, the maximum gradient adopted was 0.6%, compensated for curvature at the rate of 0.05% per degree.

The sharpest curve allowed was of 4°, all curves of 3° and more

* Presented at the meeting of October 21st, 1903.

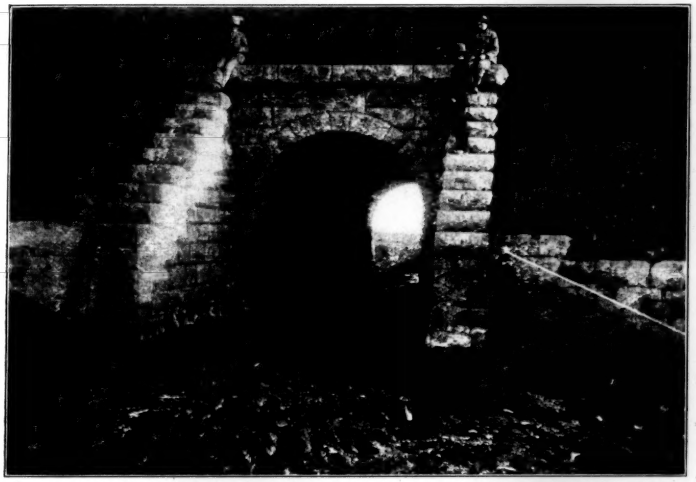


FIG. 1.—TYPICAL 10 x 6½-FOOT ARCH CULVERT.

FIG. 2.—DOUBLE 14 x 5½-FOOT ARCH CULVERT.

being spiralled by the six-chord spiral, a modification of a parabolic curve originated by Mr. J. R. Stephens, Engineer in Charge of Construction, the grading being cross-sectioned and built on the offsetted curves.

Degree of main curve.	Length of terminal curve, in feet.	Degree of terminal curve.	Terminal angle.	Offset, in feet.
3°	100	1° 30′	1° 30′	0.65
4°	120	2° 00′	2° 24′	1.25

For intermediate curves, intermediate values were taken.

The length of the terminal curve is exactly divisible by four, to avoid fractions in the chord lengths.

The terminal curve has a radius of twice the length of the main curve; the spiral is half as long again as the terminal curve, one-sixth running back on the tangent, four-sixths consuming the terminal curve, the remainder being on the main curve.

The line leaving Newkirk runs across drainage for the first 8 miles, so that some heavy grading was necessary, and a number of openings were required; for the rest of the way, to the first crossing of the Arkansas River, the location is along drainage lines presenting no unusual features.

In this first section the entire country consists of a thin soil overlying solid limestone, which can be drilled and quarried easily to any dimension, and which cuts perfectly. This stone hardens on exposure to the air, and is not shattered by frost.

The original plans called for wood-box culverts, but, on account of stone being obtainable and the line being built for heavy traffic, the Chief Engineer, James Dun, M. Am. Soc. C. E., changed to arches all the pile trestles which had sufficient headroom, and the wood boxes to masonry culverts. A contract for the masonry was let to Mr. L. W. Lewis, of Emporia, Kansas, at the following prices per cubic yard:

Bridge masonry	$5.75
Rubble masonry	3.15
Rubble arch masonry	5.25
Paving	3.15
Coping steps and ring stones	6.50
Box culvert rubble masonry	3.15

Box culvert rubble, dry	$2.50
Covering stone for box culverts	5.25
Concrete in foundations	3.00
Earth excavation above water	0.35
Loose rock excavation above water	0.50
Solid rock excavation above water	1.00
All excavation below water at cost plus 10% for use of tools and supervision	
Overhaul on all stone more than one mile from the site of the work, 40 cents per cubic yard additional to above prices for each mile after the first mile.	

The Railway Company furnished the cement, f. o. b. cars, and the contractor furnished the sand, but the company paid the actual cost for hauling the sand and cement from the cars to the site of the work. The Railway Company furnished transportation for men, teams, tools and material to and from the work.

The excavations for the foundations were commenced during the first week in May, and the last masonry was finished in October, all sand being teamed from 2 to 15 miles, at a cost varying from $1.40 to $3.25 per cubic yard.

The cement used was manufactured by the Iola Portland Cement Company, of Iola, Kansas, and showed the following tensile strengths, from tests at the factory:

NEAT CEMENT (20% WATER).

1 day	204–228	lbs. per square inch.
7 days	630–657	" "
28 "	745–761	" "

CEMENT WITH 3 PARTS SAND (10% WATER).

7 days	206–230	lbs. per square inch.
28 "	310–335	" "

Owing to continuous wet weather, and high water in the creeks, it was found almost impossible to separate wet and dry excavation, so that the entire foundation work was done by the contractor on "force account," under the close supervision of the writer and two time-keepers, each detailed to certain jobs.

TABLE No. 1.—Cost of Constructing Arches, Exclusive of Cost of Cement and Sand.

		12 x 4-ft. arch at Station 2920 + 21. See Fig. 1.		10 x 6½-ft. arch at Station 2958 + 06. See Fig. 2.		24 x 10-ft. arch at Station 2672 + 43. See Fig. 3.		Double 14 x 5½-ft. arch at Station 2761 + 33. See Fig. 4.		10 x 5-ft. arch at Station 2425 + 40. See Fig. 5.		8 x 4½-ft. arch at Station 2575 + 80. See Fig. 6.		6 x 5-ft. arch at Station 2594.	
Nature of foundation		Soapstone.		Soapstone.		Gravel and rock.		Solid rock.		Quicksand.		Solid rock.		Solid rock.	
Length of barrel		22.8 ft.		50.03 ft.		28.67 ft.		31.41 ft.		22.64 ft.		49.62 ft.		49.11 ft.	
Length of wings		12.3 "		13.50 "		31.25 "		16.75 "		13.75 "		9.25 "		7.5 "	
Height of headwalls		13.62 "		15.14 "		26.87 "		16.50 "		13.62 "		12.12 "		11.62 "	
	Unit prices.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.
Excavating foundations	.....	*	$317.44	*	$ 242.00	*	†$439.29	.....	$176.87	.....	$157.41	*	$240.89	*	$359.63
Hauling and placing grillage	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	119.90	.....	.....	.....	.....
Concrete	$3.00	81.5	244.50	.....	.....	212.1	636.30	.....	.....	102.4	307.20	.....	.....	.....	.....
Arch rubble	5.25	118.0	619.50	367.8	1 930.95	585.5	3 073.87	453.0	2 378.25	113.5	595.87	285.6	1 499.40	263.3	1 382.32
Coping, steps and rings	6.50	12.6	81.90	12.3	79.95	36.0	234.00	27.2	176.80	12.3	79.95	9.4	61.10	7.8	50.70
Overhaul	0.40	178.2	71.28	91.6	36.64	7.8	3.12	19.4	7.76	25.9	10.36	295.0	118.00	.....	.....
Dry rubble	2.50	7.2	18.00	21.1	52.75	11.3	28.25	6.2	15.50	.....	.....	14.0	35.00	12.3	30.75
Excavating and changing channel	.....	.....	40.43	.....	.....	.....	426.78	.....	.....	*	57.75	*	28.19	*	33.00
Excavating new channel	0.15	.....	.....	523.0	78.45	.....	.....	‡436.0	130.80	.....	.....	.....	.....	.....	.....
Total cost of arch	.....	.....	$1 393.05	.....	$2 420.74	.....	$4 841.61	.....	$2 885.98	.....	$1 328.44	.....	$1 982.58	.....	$1 856.40
Cost per linear foot of barrel	.....	.....	61.10	.....	48.39	.....	168.88	.....	91.88	.....	58.67	.....	39.96	.....	37.80

* Force account.
† A flood in May washed out all excavation, costing $163 to clean up foundations.
‡ At 30 cents per cubic yard.

TABLE No. 2.—Cost of Constructing Box Culverts, Exclusive of Cost of Cement and Sand.

		3 x 3-ft. dry box at Station 2397.		4 x 6-ft. dry box at Station 2404.		4 x 6-ft. rubble box at Station 2622 + 80. (See Fig. 7.)		2 x 2-ft. dry box at Station 2638 + 71. (See Fig. 8.)		Double 3 x 3-ft. dry box at Station 2710 + 08. (See Fig. 9.)	
Nature of foundation		Boulders and gravel.		Boulders and gravel.		Clay.		Soapstone.		Clay.	
Length of box		24.5 ft.		41.06 ft.		28.1 ft.		69.57 ft.		21.9 ft.	
	Unit prices.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.	Quantities, in cubic yards.	Amounts.
Excavating foundations	.....	*	$75.75	*	$150.27	*	$53.62	*	$107.14	*	$32.26
Rubble masonry in cement	3.1	.....	.....	.....	.....	79.5	252.42	.....	.....	9.0†	28.35
Paving	3.15	.....	.....	.....	.....	4.7	14.80	5.4	17.01	6.2	19.53
Concrete	3.00	15.1	45.30	21.4	64.20	.....	.....	.....	.....	.....	.....
Dry rubble	2.50	19.8	49.50	75.0	187.50	.....	.....	49.2	123.00	36.2	90.50
Covering	5.25	6.8	35.70	13.3	69.82	9.1	47.77	16.1	84.52	9.0	47.25
Coping	6.50	1.3	8.45	2.0	13.00	4.1	26.65	1.1	7.15	3.5	22.75
Excavating and changing channel	.....	.....	.....	*	8.85	.....	.....	.....	.....	.....	.....
Excavating channel	.....	.....	.....	.....	.....	.....	.....	*	29.70	.....	.....
Total cost	.....	.....	$214.70	.....	$493.64	.....	$395.26	.....	$368.52	.....	$240.64
Cost per linear foot of box	.....	.....	8.76	.....	12.02	.....	14.07	.....	5.30	.....	10.94

*Force account. †Center wall.

FIG. 1.—24 x 10-FOOT ARCH DURING CONSTRUCTION.

FIG. 2.—COMPLETED 24 x 10-FOOT ARCH CULVERT.

The cost of this excavation varied so much, on account of the different causes, pumping and floods, that it was extremely difficult to get at the exact cost per cubic yard.

An average of five different jobs, including pumping, was 45 cents per cubic yard.

The costs of the masonry, given in Tables Nos. 1 and 2, are for the excavation for the foundations, the excavation required for the channel changes, and the contract price per cubic yard for the masonry contained in each arch or box culvert, no account being taken of the sand and cement used.

The cost of the cement and sand varied with the length of the haul from the river or nearest unloading point, the sand costing from $1.40 to $3.25 per cubic yard, delivered, the larger amount being for sand used on work 15 miles from the river. The average cost of hauling the cement, for six different jobs, was $0.40 per barrel, the average load being 28 sacks, or 7 barrels. This high average was due to the wet weather and, consequently, the bad condition of the roads.

Most of the arches were founded on solid rock, soapstone or other hard material; but one, viz., the 10-ft. arch at Station 2425 + 40, was built on quicksand. In making the foundation excavation for this arch the men sank to their knees. The bottom being too soft for the regular foundation plan, the writer obtained authority from Mr. Stephens to build the masonry on a concrete base on a grillage of old piling, drift-bolted together, to form a solid floor.

This base is 52 ft. long, 20 ft. wide and 2½ ft. thick, weighing approximately 250 tons. It settled about 0.4 ft. before the masonry was commenced. There has been no further movement that could be determined.

The stone for the concrete was broken by hand into pieces small enough to pass through a 3-in. ring, the breaking being done on the site of the work. The stone for the concrete was entirely free from dirt.

The sheeting and ring stones were of the dimensions shown on the drawings, and were laid with ½-in. joints.

The joints were made on the radial lines, and the faces of the sheeting stones were hammered so as to make a close fit on the lagging. No joints closer than 12 ins. were allowed, and each stone was the full depth of the arch. The entire ring was pointed after the centers had been removed.

The cement hardened rapidly, so that it was not necessary to leave the centers in very long after the ring had been turned. Centers were never removed, however, in a less number of days after the masonry was completed than the number equaling the diameter of the arch in feet.

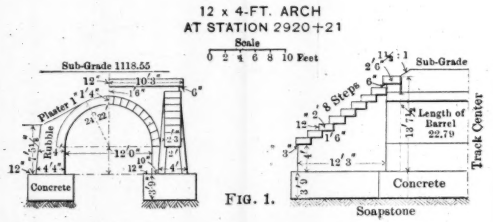


FIG. 1.

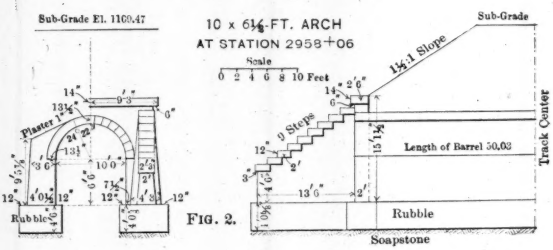


FIG. 2.

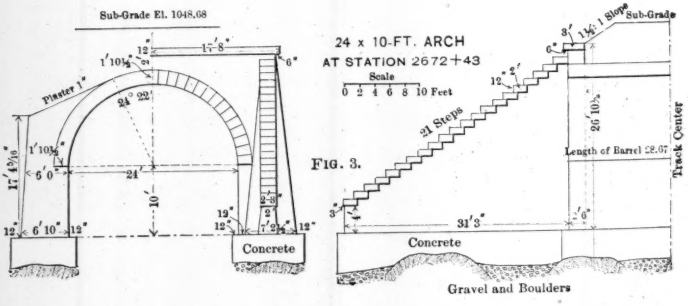


FIG. 3.

In almost every case the creek channels crossed the right of way at an angle; therefore, channel changes were made to admit and discharge the water freely. A great deal of the material from the excavation thus made was utilized in building the embankments.

FIG. 1.—DOUBLE 4 x 3-FOOT BOX CULVERT.

FIG. 2.—EXCAVATING FOR RESERVOIR SPILLWAY.

All the masonry was laid out on a grade. The writer believes in setting boxes on a grade of not less than 1 per cent. The largest arch built was set on a grade of 1.25 per cent.

The scale of wages paid was as follows:

Mason foreman	40	cents per hour.
Masons	35	" "
Quarrymen	25	" "
Mortar mixers	20	" "
Labor	17½	" "
Teams	32½ to 35	" "

The work was done on a 10-hour-day basis.

Newkirk is located on a high table land underlaid with limestone, and the water obtained from wells is too hard for boiler use and too small in quantity to supply a steam pumping plant. For these reasons the Chief Engineer decided to build a reservoir for engine water, at a place 2½ miles from town, by raising the grade line of the embankment to form a dam, thus impounding in a ravine about 93 000 000 galls. of water. The drainage area of this reservoir is 2.45 sq. miles, and the project made it necessary to excavate a new channel for Wolf Creek, 2 800 ft. long, and parallel to the railway embankment. This creek has a discharge of about 2 cu. ft. per second for about six months in the year.

The location of the high-water line made it necessary to buy outright two farms and a portion of a third, as well as change the County road. The Railway Company made 7 145 cu. yds. of embankment and built a new 56-ft. pile trestle to raise the wagon road 4 ft. above the crest of the spillway.

In building the embankment for the dam the sod was first stripped from the base and laid close to the slope stakes at the back. A core-trench 6 ft. wide and from 2 to 4 ft. deep was then dug, so as to reach the hard, impervious clay.

The embankment was put in by a steam-grader outfit, with which a wheeler outfit worked, borrowing yellow clay for the core in the center. This clay core was solidified by being trampled by men and horses and rolled continually by the wheels of the scrapers and wagons, and was carried up to the water level. The dam has a top width of 20 ft., a slope of 2½ to 1 on the water side and of 1½ to 1 on the back. It contains, approximately, 70 000 cu. yds. of earth.

DOUBLE 14 x 5½-FT. ARCH
AT STATION 2761+33

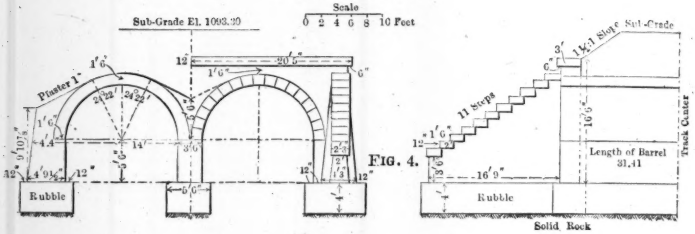


FIG. 4.

10 x 5-FT. ARCH
AT STATION 2425+40

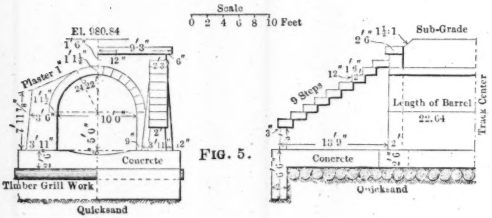


FIG. 5.

8 x 4½-FT. ARCH
AT STATION 2575+80

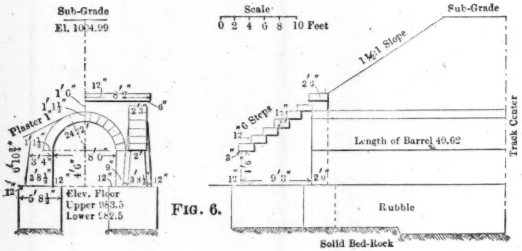


FIG. 6.

4 x 6-FT. BOX
AT STATION 2622 + 80

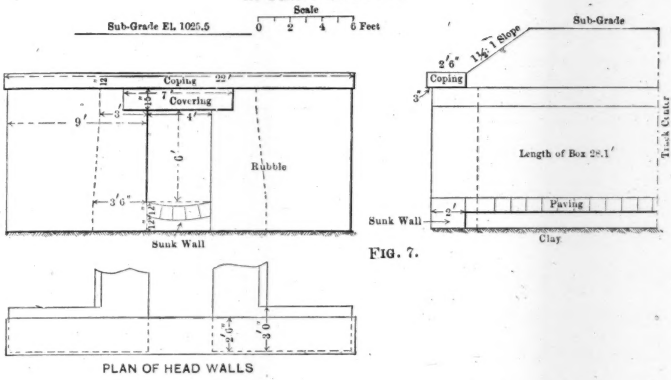


FIG. 7.

2 x 2-FT. BOX
AT STATION 2638 + 71

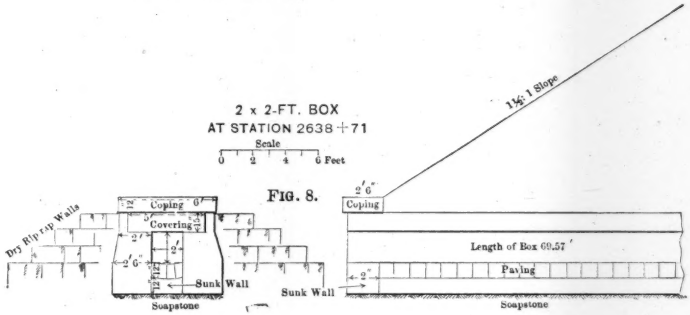


FIG. 8.

DOUBLE 3 x 3-FT. BOX
AT STATION 2710 + 08

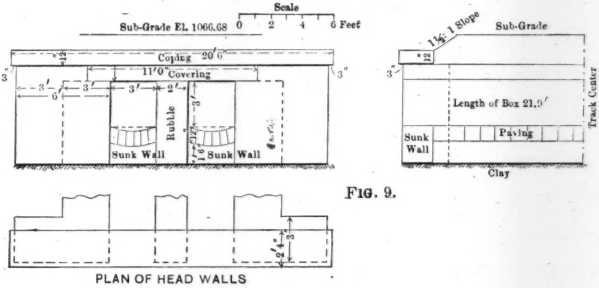


FIG. 9.

The rip-rap was composed of limestone, hauled by the construction train from a rock-cut 2 miles away. It was 18 ins. thick, and was placed on the face by hand.

The dam is perfectly water-tight, there being no seepage either through it or along the base.

The length of the spillway is 120 ft., the depth of water on the crest being calculated never to exceed 2½ ft. The length of the floor is 100 ft., and the water is discharged into a channel 20 ft. wide on the bottom and having slopes of 1 to 1, the floor being curved to keep the main current on the center line.

The construction was commenced in December, and the concreting in January. The curved sides and floor were excavated in the hard red clay on which the spillway is located, the work being done to a depth of 12 ins. below the grade stakes set for the top of the pavement. Ogee curve-forms were set for the crest wall, no trouble being caused by their moving during the progress of the work.

The concrete was made in the proportion of 1 part Portland cement, 2½ parts sand, and 4 parts crushed stone, the sand and cement being first shoveled together dry. Some difficulty was caused by the sand being partly frozen, which made it necessary to heat about one-third of the quantity used. The cost of cement, at the factory, was about $1.30 per barrel, and 576 bbls. were used.

The cost of the spillway was:

Excavation, 2 178 cu. yds.—Force account		$1 248.33
Stone	$147.20	
Sand	77.54	
Royalty on stone quarried	15.00	
Labor	985.43	
Hardware	7.70	
Carpenter making forms	13.50	
Lumber for forms and sheeting	118.60	
Blasting powder and fuse for quarrying	7.50	
Grading and filling in around walls	58.98	
		1 431.45
Total cost of excavating, grading and concreting		$2 679.78

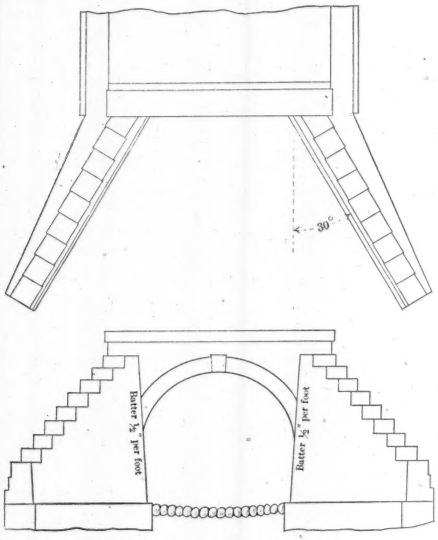
To this should be added the cost of the cement, say $750.00, thus making the total cost of the work $3 429.78.

Credit is due the contractor, Mr. L. W. Lewis, of Emporia, Kansas, and Mr. E. E. Lewis, his general foreman, for the successful handling of the work and the faithful way in which the specifications were followed.

The writer also wishes to acknowledge his thanks to his Assistant Engineer, Mr. H. S. Hancock, Jr., for his accurate instrument work and his unremitting attention to details.

DISCUSSION.

Mr. Lea. SAMUEL H. LEA, M. Am. Soc. C. E. (by letter).—This paper is of much interest, as it describes current engineering practice in railroad construction. In the writer's opinion, the method given for spiraling curves is simple and convenient, and, on this account, is preferable to most of the older and more complicated methods. Its use will simplify field operations without the necessity of resorting to tedious calculations or elaborate tables.



DESIGN FOR AN ARCH CULVERT.

FIG. 10.

The tables of cost are comprehensive, and of much interest in affording a basis of comparison for current prices for the classes of work tabulated. These tables constitute a valuable feature of the paper, and the author is to be commended for furnishing this information, especially in view of the general dearth of data of this class in engineering literature.

Judging from the photographs, the masonry, both in the arch and box culverts, is good; and the general design of the box culverts is in accordance with good practice. The writer thinks the use of bond or anchor stones in the box culvert paving would have been prefer-

able. Such stones are several inches deeper than the regular paving stones and extend entirely across the floor of the culvert. Ordinarily, they are placed at intervals of from 6 to 10 ft., in the length of the culvert, and serve to hold the pavement in place in case of its undermining or washing. Mr. Lea.

In the writer's opinion, the arch culvert designs are defective in two important features, namely:

1.—The wing-walls at the up-stream end of the culvert have their axes parallel with the axis of the culvert.

2.—A shoulder or offset is left, at the junction of each wing-wall with the trunk of the culvert, between the face of the wing-wall and the face of the side-wall of the culvert. Both of these objectionable features are eliminated in Fig. 10, which illustrates an arch culvert built under the writer's supervision.

In arch culvert construction, the writer's practice has been to build the upper wing-walls splayed at an angle of about 30° to the axis of the culvert and with their faces on line with the faces of the side-walls of the culvert at the junction of the wing-walls with the trunk. This construction requires that the faces of the culvert walls shall be battered to correspond with the batter of the wing-walls. In this design there are no shoulders or re-entrant angles at the upper end of the culvert below the springing line of the arch. The water is afforded a free and unobstructed entrance to the culvert, and there are no projecting surfaces to catch floating drift and débris.

EMILE LOW, M. Am. Soc. C. E. (by letter).—It is to be hoped that this paper will elicit full discussion, especially as so many railroad engineers are enrolled in the Society. Some years ago a somewhat similar paper was presented, and the writer was the only one who took part in the discussion. It is difficult to explain why so few members contribute to the discussions. It may be from lack of interest, or perhaps railroad engineers think they are too busy. Mr. Low.

If a map of the line accompanied the paper it would be very advantageous, as the alignment and profile of a railway line are always an interesting study, particularly to the locating or constructing engineer. The character and magnitude of the grading are grasped more readily when thus graphically presented.

Mr. Allan gives quite a number of illustrations and plans of arch- and box-culverts, but it is to be regretted that he has not stated the manner or method by which the sizes of the various openings were determined.

As most engineers are aware, the determination of culvert openings is far from scientific. It would not be far from the truth to say that in most cases they are determined by guesswork, and that much depends on the judgment of the guesser.

One bad feature of the culverts, as built, is the offset at the junction of the wing-walls and the barrel, the tendency of which is to

Mr. Low.

TYPICAL SECTION OF 16-FOOT ARCH-CULVERT.

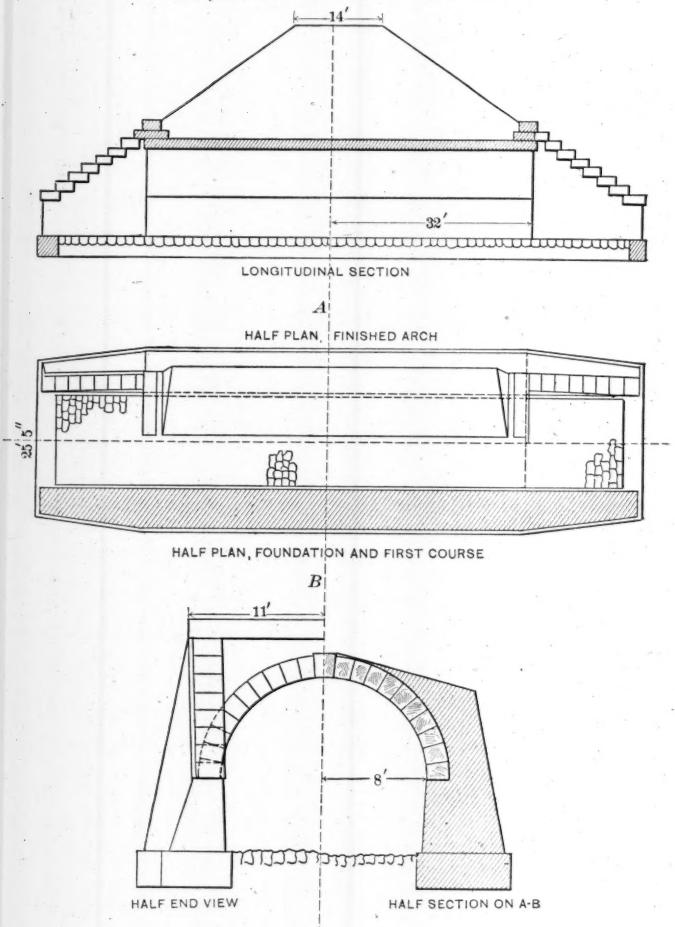


FIG. 11.

contract the waterway. It is also a place for the lodgment of drift-wood. Also, would it not have been better to build a single arch, of a somewhat smaller waterway, than the double one? Mr. Low.

One good feature is the furnishing of the cement by the railroad company. This is the practice of a number of first-class railroads, and has much to commend it.

Fig. 11 is a typical plan of an arch-culvert, indicative of the practice of a number of prominent eastern railroads, and is the type which the writer prefers.

This plan shows parallel wing-walls, but flaring walls are often to be preferred. In every case, however, the offset at the junction of the walls and the barrel is avoided.

H. F. DUNHAM, M. Am. Soc. C. E. (by letter).—It would be interesting to trace by the aid of sketches and photographs the gradual change from difficult to simple designs for wing-walls for arch bridges. Every engineer familiar with early railway construction in this country has felt a thrill of genuine pleasure as well as astonishment when standing upon the parapet of a high masonry bridge above the amphitheater formed by wing-walls that follow for 90° the circumferences of circles having for their radii the "distance out" and for their centers points in the lines of the bench-walls extended. The effect is intensified if the broad area is well covered by large forest trees. But when it is necessary to widen the roadbed and increase the number of tracks—that invites a different state of mind. Mr. Dunham.

The wing-walls should be built in the line of the culvert walls, and begin to batter, if they are battered at all, at the skewback or springing line of the arch. There may be instances in cities and conspicuous places where, for appearance, the design shown by the author is warranted, but, generally speaking, and especially in the West, where it may not be quite certain whether a new road will pay 11% or only 10% upon the investment, any change that makes the work less expensive and stronger, while adding to the safety, should invite attention.

Offsets and corners in masonry are sources of annoyance and expense that the contractor usually "figures in." Offsets at the end of a culvert invite accumulations of drift; so do wedge- or funnel-shaped passages, while the straight-wall wings offer at their ends the same width of passageway that the culvert offers, and if flood-wood, fence rails, or what not, stop there, causing the water to dam up and overflow, the overflow goes through the culvert and will continue to do so after such dam has reached a height above the top of the culvert opening; so that the straight wing, instead of embracing trouble, is a rude fender to keep trouble away. Especially is this true of a small opening under a high embankment.

The parapet and coping are diminished by such construction.

Mr. Dunham. Extension of the culvert becomes an easy matter, and, as for appearance—there is a possibility of getting used to it.

To illustrate further these advantages, Plate XV is introduced, and shows a culvert, built in 1890, on the line of the Wheeling Bridge and Terminal Company, now owned by the Pennsylvania Railway. The Chief Engineer was the late Job Abbott, M. Am. Soc. C. E., and the writer was Engineer in Charge.

Mr. Snow. J. P. SNOW, M. Am. Soc. C. E. (by letter).—The writer wishes to correct the statement made by Mr. Samuel H. Lea, that the construction of arch culverts, with splayed wings and no set-back of the wings at the face of the arch, "requires that the faces of the culvert walls shall be battered to correspond with the batter of the wing-walls." No such necessity exists. The faces of the walls of the trunk may be built plumb, as many engineers prefer, on account of increased bottom width, and the splayed wings built at any desired batter.

The work must be set out so that the line of intersection of the two planes, one vertical and the other inclined, will pierce the plane of the face of the arch at the springing line. This is done readily by using the horizontal element at the springing line level by which to stake out the wing.

This construction is no more difficult than that described by Mr. Lea, and it looks all right when finished. The intersection of the two faces inclines boldly against the face of the arch with the effect of a brace.

The elimination of the shoulder, objected to by Mr. Lea, is certainly very desirable, although, of course, it involves a lack of bond between the wing and the trunk for the few courses where the wing crosses the arch ring. If the arch is segmental instead of full center, as shown by the author, this distance, where bonding is impossible, will be shortened and the work cheapened; because, for small arches, the ordinary sizes of stone are deep enough for sheeting, and the arc will be shorter if segmental than if semicircular.

Mr. Allan. A. G. ALLAN, Assoc. M. Am. Soc. C. E. (by letter).—Replying to Mr. Low's question: The method by which the various culvert openings were determined was as follows. When the line was finally located, a ravine section for each drainage crossing, on a scale of 10 ft. to 1 in., was made by the locating party, from which the Chief Engineer determined the class of structure to be used. Each drainage area was run by the locating party and then the areas were put on the ravine sections.

After the kind of bridge opening had been determined, the drainage areas were re-run and checked by the Resident Engineer in Charge of Construction, using the transit and measuring distances by 35-minute deflections. These areas were platted on the map of the

CULVERT ON THE LINE OF THE WHEELING BRIDGE AND TERMINAL COMPANY.

line, and not only were all the openings, up to about 10 sq. miles, checked by this method, but the drainage areas of all side-ditches, carrying water parallel to the track, were surveyed and platted on the drainage map. Mr. Allan.

Large areas were usually determined from the maps of the United States Geological Survey, although the writer has known of several doubtful areas which were surveyed, and each of which took a party a week to go around.

The drainage tables used (Table No. 3) are the standard tables of the road, and were computed by the Chief Engineer, James Dun, M. Am. Soc. C. E. The writer believes that these were computed from long observations of actual run-offs along the oldest railroad line in Missouri. The various States traversed by the railroad have different percentages of the amounts shown, Oklahoma using 100 per cent.

TABLE No. 3.—APPROXIMATE AREAS OF WATERWAYS FOR DRAINAGE AREAS OF 0.1 TO 6 000 SQUARE MILES.

Area drained, in square miles.	Area of waterway, in square feet.	Box and Arch Culverts.		Area drained, in square miles.	Area of waterway, in square feet.	Arch Culverts.		Area drained, in square miles.	Area of waterway, in square feet.	Area drained, in square miles.	Area of waterway, in square feet.	Area drained, in square miles.	Area of waterway, in square feet.
		Diameter, in feet.	Bench, in feet.			Diameter, in feet.	Bench, in feet.						
0.01..	2.0	2	1 Box.	1.5..	150	14	7	11..	710	110..	2 220	1 200..	6 960
0.02..	4.0	2	2 "	1.6..	160	16	6.5	12..	740	120..	2 315	1 300..	7 230
0.03..	6.0	2	3 "	1.7..	170	16	7	13..	775	130..	2 405	1 400..	7 480
0.04..	7 5	2.5	3 "	1.8..	180	16	7.5	14..	805	140..	2 500	1 500..	7 725
0.05..	9.0	3	3 "	1.9..	190	16	8	15..	835	150..	2 580	1 600..	7 960
0.06..	10.5	3.5	3 "	2.0..	200	18	7	16..	865	160..	2 665	1 700..	8 195
0.07..	12 0	3	4 "	2.2..	220	18	8	17..	890	170..	2 745	1 800..	8 390
0.08..	13.5	D 2.5	3 "	2.4..	240	18	9.5	18..	920	180..	2 820	1 900..	8 625
0.09..	15.0	" 2.5	3 "	2.6..	260	20	8	19..	945	190..	2 900	2 000..	8 820
0.10..	16.0	" 3	3 "	2.8..	280	20	9	20..	970	200..	2 970	2 200..	9 240
0.15..	25.0	" 3	4 "	3.0..	300	20	9.5	22..	1 015	220..	3 115	2 400..	9 605
0.20..	32.0	6	4 Arch.	3 2..	320	22	8.5	24..	1 060	240..	3 245	2 600..	9 970
0.25..	38.0	6	5 "	3.4..	340	22	9	26..	1 100	260	3 370	2 800..	10 320
0.30..	44.0	6	5.5 "	3.6..	357	24	8.5	28..	1 140	280..	3 495	3 000..	10 640
0.35..	51.0	8	4.5 "	3.8..	373	24	9	30..	1 180	300..	3 615	3 500..	11 445
0.40..	56.0	8	5 "	4.0..	388	28	7	32..	1 220	325..	3 770	4 000..	12 160
0.45..	62.0	8	6 "	4.2..	403	28	7.5	34 .	1 255	350..	3 900	4 500..	12 825
0.50..	66.0	8	6 "	4.4..	417	28	8	36..	1 290	375..	4 035	5 000..	13 500
0.55..	70.0	8	6.5 "	4.6..	430	28	8.5	38..	1 320	400..	4 165	6 000..	14 520
0.60..	74 0	10	4.5 "	4.8..	443	28	9	40..	1 350	450..	4 385		
0.65..	78.0	10	5 "	5.0..	455	28	9,5	45..	1 435	500..	4 610		
0.70..	81.0	10	5.5 "	5.5..	483	28	10.	50..	1 510	550..	4 825		
0.75..	85.0	10	6 "	6.0..	509	32	7.5	55.	1 580	600..	5 030		
0.80..	88.0	10	6.5 "	6.5..	533	32	8	60..	1 650	650..	5 230		
0.85..	91.0	10	6.5 "	7.0..	556	32	9	65..	1 720	700..	5 420		
0.90..	94 0	10	6.5 "	7.5..	579	32	10	70..	1 780	750..	5 610		
0.95..	97.0	12	5 "	8 0..	601	32	11	75..	1 840	800..	5 800		
1.00..	100.0	12	5 "	8.5..	622	32	11.5	80..	1 900	850..	5 890		
1.1 ..	110.0	12	6 "	9.0..	641	32	12	85..	1 960	900..	6 080		
1.2 ..	120.0	12	7 "	9.5..	660	32	12.5	90..	2 015	950..	6 230		
1.3 ..	130.0	12	8 "	10.0..	679	32	13	95..	2 065	1 000..	6 330		
1.4 ..	140.0	14	6.5 "					100..	2 120	1 100..	6 705		

Mr. Allan. The number of square feet required is also checked by high-water marks, the highest being placed on the ravine sections by the locating engineer.

On the main line, where records of the floods extend over a period of years, many bridges will be found having a series of high-water marks, with their respective dates, put on with paint by the section foreman or the bridge department. These distances below the base of the rail are also placed in the bridge record kept by the Resident Engineer.

In modern railway construction, the determination of the size of a culvert or bridge opening is certainly not a method of guesswork.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

INSTITUTED 1852.

TRANSACTIONS.

Paper No. 968.

THE FATIGUE OF CEMENT PRODUCTS.*

By J. L. Van Ornum, M. Am. Soc. C. E.

With Discussion by Messrs. E. R. Buckley, L. F. Bellinger, A. L. Johnson, H. F. Dunham and J. L. Van Ornum.

Although the investigation has only begun, the writer believes that the importance of the subject makes it advisable to state the results (as far as obtained), of experiments made under his direction, to determine the behavior of certain Portland cement mixtures when subjected to repeated loads of a less intensity than would produce failure when once applied. Certain observed peculiarities in tests of concretes led to the conclusion that brittle engineering materials (such as stone, brick, mortars, concretes, etc.), of which cement mixtures are a fair type, possess the property of a progressive failure or "gradual fracture" which finally becomes complete under the repetition of a load well within the ultimate strength of the material. The analogy to the similar property of steel, proved by Bauschinger and Wöhler about forty years ago, is evident.

The probability of this fact was indicated to the writer last December, when some peculiar results of an investigation made on concretes at Washington University could only be explained, apparently, on the hypothesis already mentioned. The time which has since elapsed has

* Presented at the meeting of October 21st, 1903.

been too short for the extended investigation necessary to develop details, but results thus far obtained indicate plainly that this characteristic is true.

The experiments made were compressive tests upon neat Portland cement blocks (approximately 2-in. cubes), using a standard American brand, and crushed when four weeks old. The tests were made by Mr. Hans Schantl, of the civil engineering department, an expert experimenter. The ultimate strength was determined in the usual way, and similar blocks were subjected to certain percentages of the ultimate strength, varying from 95 to 55% of the same, the load in each case being applied and removed repeatedly until failure occurred. In all, ninety-two blocks were tested, and the average results are indicated graphically in the accompanying diagram, Fig. 1.

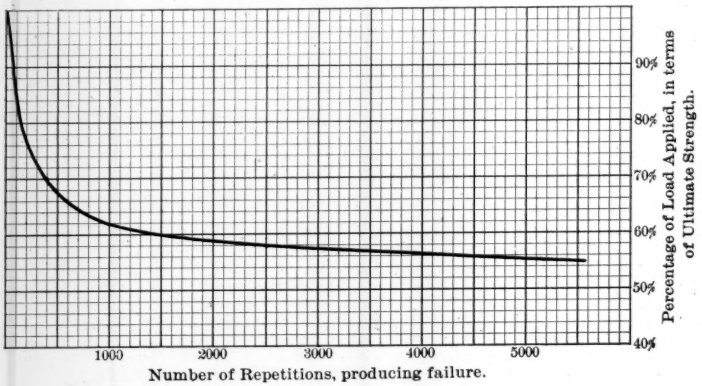


FIG. 1.

In general, the figure indicates that failure occurs under repeated loadings of much less intensity than that necessary to cause failure when once applied (the latter being the test always relied upon to judge of the strength of such a material), and that the number of repetitions necessary to cause rupture is greater for the smaller loads, as would be expected. This curve should be considered characteristic, and not precise; it does not indicate necessarily the exact relation between the number of repetitions and the corresponding intensity of load, but rather typifies the general law of "fatigue" as outlined

above. It should also be said that while time has not yet permitted similar systematic tests on concrete, yet eighteen repetition tests on 7-in. cubes indicate by their behavior that the same general law of gradual failure applies equally to concretes.

It is to be hoped that this investigation may be extended as rapidly as circumstances will allow. In determining the proper factor of safety to use, for concretes under various conditions of design, such facts would be especially pertinent. Concretes vary so widely in character and quality of materials, proportions of ingredients, and in characteristics depending upon age, conditions of loading, whether or not reinforcing metal is introduced, etc., that much time and labor will be necessary to establish the law of "fatigue" under the various conditions existing. Especial care is also necessary because of the difficulties inherent in securing definite results with cement products.

If a number of research laboratories should become interested in developing the actually existing laws and relations indicated, the securing of the useful results will be expedited.

A certain portion of this research has been undertaken by Washington University, and the writer has planned a series of experiments which will continue throughout the year in its testing laboratory. It is the intention to make public the results, as definite facts and the principles of behavior of these materials under repeated loads are developed.

DISCUSSION.

Mr. Buckley.

E. R. BUCKLEY, Esq.* (by letter).—The experiments to determine the fatigue of cement products, which are being carried on in the testing laboratory of Washington University, under the direction of Professor J. L. Van Ornum, are extremely interesting and practical. Professor Van Ornum has started a line of investigation which has heretofore been practically overlooked. It is hoped that he may be able to procure data showing some uniformity in the rate of weakening of cement products under repeated loading. It is thought, however, that a series of experiments showing the rate of weakening under a constant load below the ultimate strength would perhaps be of even greater commercial importance than the experiments now being conducted.

In 1898, the writer recognized the same phenomenon of fatigue, in stone, while making a series of tests, in the preparation of a report on the "Building and Ornamental Stones of Wisconsin." On pages 367 and 368 of that report, will be found the following observations:

"No data are thus far available from which one can glean any knowledge of the effect of long continued pressure below the point of rupture on the strength of the rock. Neither have data been obtained indicating the effect of intermittent stresses (repeated loading) on the rupturing of rocks. In making the compressive strength tests in the preparation of this report, a number of samples were placed in the 100 000-lb. Riehlé testing machine, which could not be crushed. These samples were removed, and afterwards crushed in a 300 000-lb. machine, with the results indicated in the accompanying table. The apparent loss in strength in some of the samples cannot be entirely attributed to the intermittent character of the pressure. It is probably due, to some extent, to the fact that the samples were chipped at the corners and edges when in the first machine. These and other causes incident upon a change of machines may partially account for the losses, but, nevertheless, the results would seem to indicate that an actual decrease in strength is occasioned by continued or intermittent pressure below the ultimate strength. Further experiments along this line are needed."

Four of these tests gave an increased resistance in the larger machine, while three of them exhibit a decrease of several thousand pounds per square inch.

In connection with the preparation of a report on the building stones of Missouri, the writer contemplates verifying, if possible, the conclusions reached in his Wisconsin report relative to the fatigue of stone.

It is believed that these experiments will be fruitful of results which will have a commercial, as well as a scientific, value.

The following is the table referred to in the paragraph quoted:

* Director, Missouri Bureau of Geology and Mines.

Mr. Buckley.

"TABLE SHOWING THE DIFFERENCE IN CRUSHING STRENGTH THROUGH INTERRUPTED PRESSURE."

Name of Quarry.	CRUSHING STRENGTH.	
	100 000-lb. Machine.	300 000-lb. Machine.
Pike River Granite Co.	25 000	27 887
New Hill O'Fair (Granite)	25 000	27 262
Milwaukee Monument Co. (Granite)	25 000	38 063
Lenthold Quarry (Granite)	25 000	20 875
Washington Stone Co. (Limestone)	27 855	29 526
Gillen Stone Co. (Limestone)	25 000	20 308
Gillen Stone Co. (Limestone)	24 783	14 869

Mr. Bellinger.

L. F. BELLINGER, M. Am. Soc. C. E. (by letter).—This paper, with its interesting diagram, will be certain to attract considerable attention from designers in concrete, as it is the first attempt to place on a scientific basis certain phenomena which have no doubt been noticed by all engineers performing laboratory tests and full-size tests of the materials mentioned.

The nomenclature of the paper includes "progressive failure" and "gradual fracture" under the heading of "fatigue." The latter seems to be a good term for neat-cement tests which fail by tensile stress, below the ordinarily determined ultimate strength, after a short time. The writer has in mind some 7-day tests of neat Portland cement briquettes which were tested in a Riehlé machine having a capacity of only 1 000 lbs. The briquettes were left with the stress of 1 000 lbs. per square inch on them until they broke. The length of time varied considerably, from zero to one minute, and one briquette carried that load for $1\frac{1}{2}$ minutes before failure occurred. This type of failure might be called "fatigue" in the strictest sense. So also might be called the failure of full-size floor slabs of reinforced concrete construction. Seven of these floor slabs were tested under the writer's supervision, with the following general results: Each test floor consisted of three bays, of 6 ft. span and 5 ft. width, the center bay only carrying the test load of pig iron, concentrated along its center line. The reinforcement of the concrete was Clinton wire-cloth, or expanded metal of various sizes. In general, it was noted that the load which caused the first noticeable deflection, $\frac{1}{32}$ to $\frac{1}{16}$ in., was just about one-half of the load which caused the failure of the concrete; that is, the cracking of the concrete at the haunches or the middle of the span, exposing the metal. It was noted that the workmen could keep piling up the pig iron after the actual failure until the load was 20% more, before the load fell through the floor, rupturing the steel. A further important note was, that the load at actual failure, as noted above, if left alone, caused the spalling off on the bottom of the slab to con-

Mr. Bellinger. tinue more or less rapidly, indicating the destruction of the floor in time, through "fatigue" of the materials.

When it comes to compression tests, there is another and older term than "fatigue" describing the action of material under test quite fully, and that is "disintegration." It is difficult to get a good uniform bearing for crushing brittle material. If the plates at the jaws of the testing machine are hard, some high point of the specimen is crushed or split off before the remainder receives its share of the load; if the plates are soft, the material flows into the pores of the test piece, and, acting like a wedge, pushes off a few grains at a time, or destroys the cohesion between the grains, a little at a time, until the specimen fails. Examples of repetition of small applications of stress are: The use of stone hammers to split rock; the wearing away of rocks by rolling in the beds of streams, and by the action of river currents; the disintegration of concrete sea-walls, between low water and high water, by wave and ice action, and, when the coarser particles are reached, by the freezing of water between these particles.

The analogy between the "fatigue" of cement in compression and the "disintegration" of concrete, stone, etc., in the examples cited is plain. In each case it is a question of the number of applications of stress, by machine, freezing, impact of ice cakes, etc., and the amount of this stress at each application. This question is the one the author has begun to answer, and, from the looks of his curve and the direction of its lower end, we will soon have the stages of failure of brittle materials, from direct crushing down to "drops of water wearing away the hardest stone." It will be a long and tedious investigation, and the experimenters should be encouraged by all.

Mr. Johnson. A. L. JOHNSON, M. Am. Soc. C. E. (by letter).—The writer is much interested in the preliminary results, obtained by Professor Van Ornum, on the fatigue of cement. It is a matter of prime importance, and, in the writer's opinion, should be investigated thoroughly and carefully. If it is a fact that concrete and steel-concrete structures will fail with infinite repetition, under one-half the load required for single application, it is a matter that must be taken care of at once in the factor of safety used in designing.

Considère has reported that "concrete tends to yield somewhat under a soliciting force." It is the writer's experience that it also tends to recover when the load is removed, and, therefore, he would suggest that, in all probability, no such tremendous decrease in strength, under continued application, would be found, if a considerable period of rest were allowed between applications, which is much nearer the condition in practice.

The writer is also of the opinion that, while, no doubt, the curve is asymptotic to a horizontal line representing some percentage or other, it should cross the vertical axis at right angles; the curve,

in his judgment, necessarily having a maximum point on this line. This would make the curve of the nature of the "Witch of Agnesi." Of course, this is a matter of detail that would be brought out in the full investigation of this subject, which the writer trusts will be continued, either by Professor Van Ornum, or others, or a number of laboratories working in conjunction, in the matter, to a final conclusion. Mr. Johnson.

In relation to this, he would like to suggest that the subject of steel-concrete construction is so broad that it is practically impossible for any university alone to investigate the subject thoroughly, and he believes that the only practical plan would be for the professors of civil engineering to get together and subdivide this work among different institutions having well-equipped laboratories, having the specimens made by men of experience, rather than by students. In this way, in the course of a year or two, conclusive information of the greatest value might be hoped for, in a line of engineering construction rapidly assuming a position of importance equal to that involving the use of any other engineering material.

H. F. DUNHAM, M. Am. Soc. C. E. (by letter).—This paper would be of greater interest to those not familiar with the subject if the author had outlined the method by which repetitions were made. Anyone familiar with tests of cement mortar or concrete knows how difficult it is to subject a test piece to the same conditions two or more times in succession. If there could be any way of actually delineating the complex stresses produced in the piece the first time, something more subtle than the theory of probabilities might be required to convince one that those stresses would ever be exactly reproduced or repeated in subsequent tests. The question naturally arises whether or not some part of the sample was not subjected to a greater stress when under 90% of the breaking load than it was when 95% of that load was applied. How much can be actually known, then, in regard to the stresses set up in another sample? Or what assurance can there be that the loads exacted 98 or 95 or even 90% of the ultimate strength? Very little mortar or cement in good work is ever required to bear as much as 50% of its ultimate load, and 20 or 25% would be more frequently, though seldom, imposed. What effect upon the material has repetition of such stresses? Those who have used cement in the construction of tall stacks or chimneys, where it is subjected to very heavy pressures and where the load must vary with every prairie wind that blows, and have found the mortar apparently harder after twenty years than it was after one year had passed, will appreciate the gradual inclination of the curve in its lower percentages. Mr. Dunham.

Fatigue under repeated applications of 95% of the greatest possible load may not be irreconcilable with an increase of strength

Mr. Dunham. under repeated applications of 25% of the same load. It is not—in the case of a man.

Mr. Van Ornum. J. L. VAN ORNUM, M. Am. Soc. C. E. (by letter).—Concerning the methods of making the tests, it may be said that the specimen is placed in the ordinary type of Riehlé testing machine between spherical bearing plates (for equalizing pressure) after grinding the ends to plane surfaces. The load is applied by the hydraulic pump, and then released when the weighing beam indicates the required load. In order that the repetitions may be applied under entirely similar conditions, it is necessary that a certain minimum load should remain; else the specimen would be displaced. This minimum load, in these tests, is about 4% of the ultimate strength of the specimen.

About four repetitions are made per minute. The loads are applied and removed as rapidly as is convenient, because of the question of time. This will be appreciated when the number of repetitions runs into the thousands, and, further, when it is considered that specimens one month old are naturally getting stronger; a fact which would disorganize the "percentage" factor if the tests were continued through many days. There is pertinence in the suggestion of Mr. Johnson that a considerable period of rest between applications of load might give very different results. The writer believes that such comparative tests must be made on specimens at least a year old, so that the ultimate strength may have reached a relatively permanent amount. However, the few experiments that give any clue to the effect of the time factor, such as those cited by Dr. Buckley and some by the writer, seem to indicate that the results may not be so very different as might be thought.

Undoubtedly, Mr. Johnson's opinion, that the curve should be of some cubic or transcendental form, which will give a maximum value having a horizontal tangent at the 100% load, is theoretically correct. Whether the difference will be enough to show, on the scale used for the diagrams, is a question that will take further experimental work to determine.

The point raised by Mr. Bellinger, of the desirability of terming the failure of cement products under repeated loads a "disintegration," would seem questionable. There is no impact effect such as occurs in many of the cases cited by him as illustrating failure by disintegration. Neither is there a concentration of pressure successively on certain portions or particles of the material, causing it to fail characteristically by such progressive attrition or attenuation. Apparently, it is a question of nomenclature as modified by usage, and it is believed that the use of the word disintegration would be unfortunate.

The experiments on concrete prisms, now in progress, are so par-

ticular that conclusions cannot be given definitely for some months. Results of a large number of tests indicate that the curve for a 1 : 3 : 5 Portland cement concrete will not differ greatly in character from the curve shown in Fig. 1 for neat cement blocks. Mr. Van Ornum.

The writer is confident that engineers will appreciate the contributions of those who have discussed this paper, especially as it concerns a subject on which there is so little known.

MEMOIRS OF DECEASED MEMBERS.

FREDERICK WINN BOND, M. Am. Soc. C. E.*

DIED JULY 12TH, 1903.

Frederick Winn Bond was born of New England parentage in Dover, New Hampshire, on April 14th, 1852. The family moving to Waltham, Massachusetts, shortly after, his early education was obtained in the public schools of that place. He attended college at Yellow Springs, Ohio.

Mr. Bond began his engineering career in 1870 as a rodman on the St. Louis, Council Bluffs and Omaha Railroad, now a part of the Wabash System. He was afterward employed as a draftsman in the City Engineer's office at Omaha, Nebraska. In the year 1872 he was employed as a rodman and levelman on the Union and Southern Pacific Railways. From there, in 1873, he went to the City Engineer's office at Toledo, Ohio, but during the same year returned to Omaha and was employed as an Assistant Engineer in the United States Engineer Department at that place. During this period he was connected with the "Hayden Geological Survey," of the Territories, operating in the then little known Yellowstone country, an experience which he often spoke of in later life as very interesting.

In 1876 he was engaged as mechanical engineer in a Chemical and Blast Furnace Works at Bangor, Michigan, where he remained until 1879, at which time he became an Assistant Engineer on the St. Louis and San Francisco Railway, under James Dun, M. Am. Soc. C. E., then Chief Engineer of that railway, with general headquarters at St. Louis, Missouri. He remained with this company until the year 1889, during which time, in the years 1887 and 1888, he made extensive surveys through the Indian Territory and Texas Panhandle for the location of the Middle Division of the Atlantic and Pacific Railroad, then jointly owned by the Atchison and San Francisco interests. Leaving the St. Louis and San Francisco Railway in 1889, he became Chief Engineer of the Kansas City, Nevada and Fort Smith Railroad, now a part of the Kansas City Southern System. He remained there until 1891 when he accepted the position of Chief Engineer of the Kansas and Texas Coal Company of St. Louis, which position he held until 1897. During parts of 1897 and 1898 he was engaged in special work, for the Engineer Department of the Atchison, Topeka and Santa Fé Railroad Company, in Texas. Early in 1898 he became Principal Assistant Engineer of the St. Louis and Oklahoma City Railroad, now a part of the St. Louis and San Francisco System. In 1899 he was appointed Chief Engineer of the "Frisco" Railroad,

*Memoir prepared by R. L. Van Sant, M. Am. Soc. C. E.

operating department. In 1900 he resigned that position to become Chief Engineer of the Fort Smith and Western Railroad, which position he held at the time of his death.

The existence of the latter corporation is due primarily to Mr. Bond's foresight and business enterprise in locating and securing leases on valuable coal properties in the Indian Territory; these concessions, later, being transferred to the present railroad corporation. Much of Mr. Bond's time for the past eight years was given to this enterprise, and actual construction was finally begun some two years ago under his supervision as Chief Engineer.

While connected with the Kansas and Texas Coal Company, Mr. Bond made many innovations and improvements in coal mining and hoisting machinery, some of which were patented by him and are now in general use.

The writer's acquaintance with the deceased began at the time he came to the "Frisco" Railroad in 1879, being associated together in the location and construction of important extensions of that road during the stirring days of the early eighties. The deceased's acquaintance and railroad experience during this period extended over Southern Missouri, Northern Arkansas and the Indian Territory, in connection with the above-named extensions. He made friends with all, and left pleasurable reminders of his strong personality, genial disposition, bright wit, and, occasionally, sharp sarcasm, wherever he "pitched his tent." He was genial and generous with his friends, but his wit was often very pointed when dealing with those who opposed him. There was scarcely a man on the "Frisco" Railroad, from the Road Department to the General Office, who did not know and admire "Fred Bond." All, from the lowest to the highest express genuine regret at his untimely death, and extend sympathy to the bereaved family.

Mr. Bond belonged to that practical and resourceful class of engineers who got their experience during the last quarter of the past century under conditions calculated to develop a high order of resourcefulness and self-reliance, together with a promptness to act and assume responsibility, that is hardly possible under present conditions of organization and methods of practice. Being possessed of an eminently practical turn, yet he had much of the artistic instinct, and felt and appreciated all the better impulses of human nature. "Fred Bond" was a most delightful companion, a good comrade and a stanch friend, as well as an engineer of high professional qualifications.

Mr. Bond was married to Miss Elizabeth Loup, in June, 1875, at Belvidere, Illinois, and she and four children survive him. He died at Fort Smith, Arkansas, on July 12th, 1903, and was buried in the family lot, at Kirkwood, St. Louis County, Missouri.

He was elected a Member of the American Society of Civil Engineers on December 7th, 1898.

JOHN BUTLER JOHNSON, M. Am. Soc. C. E.*

DIED JUNE 23D, 1902.

John Butler Johnson was born, on June 11th, 1850, of Quaker parentage, on a farm near Marlborough, in the northeastern part of Stark County, Ohio. He died fifty-two years afterward, June 23d, 1902, at his summer home at Pier Cove, near South Haven, on the east shore of Lake Michigan, having in that time won a very high place as an instructor in engineering and in the esteem of all who knew him.

The first sixteen years of his life were spent at home in the wholesome life of the farm. Meantime, he attended school at Marlborough, during one year at a high school of which Dr. T. C. Mendenhall was the principal. In 1860 the family—father, mother and seven children—moved to Kokomo, Indiana. Here, for a short time, he attended the Howard College, after which he attended the Holbrook Normal School at Lebanon, Ohio.

After this, four years were spent in teaching school at various points, mostly in Indiana, though for one term he had charge of a school in Arkansas. In 1872, when twenty-two years of age, he was called to Indianapolis, where he remained two years, as Secretary of the School Board and Instructor in the High School.

In 1874 he entered the University of Michigan, from which he was graduated in the civil engineering course in 1878. His next five years were spent in the practice of his new profession, the first three years on the Survey of the Great Lakes, the last two years as an Assistant Engineer of the Mississippi River Commission, with headquarters at St. Louis. A large part of this time was spent in field work, and he became quite expert as an instrumentman, particularly in the work of precise leveling.

In April, 1882, he became a Member of the Engineers' Club of St. Louis, in which he took a very active interest. In February, 1883, he was chosen Secretary of the Club.

In the autumn of 1883 he was called to the Chair of Civil Engineering in Washington University, at St. Louis, a position in which he succeeded Professor Charles A. Smith who had also preceded him as Secretary of the Engineers' Club. For his new work, Professor Johnson's experience had been a most admirable training, and he soon attained marked success. Few indeed have been his equals in the ability to interest and inspire his pupils.

During the sixteen years in which he remained at Washington University, Professor Johnson, who was a tireless worker, found time for the writing of the books which have made his name familiar to all students of engineering. The magnitude and importance of his work

* Memoir prepared by Robert Moore, Past-President, Am. Soc. C. E.

as an author is shown by the following list of his books, given in the order of their first publication:

1.—In 1885, "Topographical Surveying by means of the Transit and Stadia."

2.—In 1886, "The Theory and Practice of Surveying." This is one of the best books of its kind, and numerous editions have been called for.

3.—In 1892, "The Theory and Practice of Modern Framed Structures," a quarto book, of 525 pages, on bridges and roofs, in the preparation of which he was assisted by Professor F. E. Turneaure and Mr. C. W. Bryan.

4.—In 1895, "Engineering Contracts and Specifications." As a preparation for this work, Professor Johnson attended a course of lectures at the University Law School.

5.—In 1897, "Materials of Construction," in which were embodied the results of a large amount of work done by him in the testing laboratory of the University.

In addition to the labor of writing these books, to say nothing of numerous professional papers and addresses, he did a large amount of work upon the "Index to Current Engineering Literature" published in the *Journal of the Association of Engineering Societies.* Of this index, which is of great value to engineers and engineering students, Professor Johnson was the originator and the manager from October, 1884, to December, 1895, when the work was taken up and continued by the *Engineering Magazine.*

From 1892 to 1895 he was engaged in making investigations of the strength of timber for the Division of Forestry of the United States Department of Agriculture. The results of these investigations, some of which were important, are embodied in several bulletins of the Department.

Professor Johnson was one of the founders, for two years the first Secretary, and for one year President of the Society for the Promotion of Engineering Education, organized in 1893. In 1895 he took an active part in the formation of the St. Louis Railway Club, a large and flourishing organization of men engaged in railway operation, and was a member of the Executive Committee until his removal from St. Louis.

In 1899, after sixteen years of service at Washington University, he accepted an appointment as Dean of the Engineering School of the University of Wisconsin, at Madison. His life there was a continuation of that at St. Louis, a life of diligent, faithful and highly successful work. Under his leadership the Engineering School at Madison took fresh life and grew as it never had done before. Meantime, he, himself, became an important factor in the life of the University and in the city of his adoption. Everything, indeed, pointed to a long

career of distinguished usefulness, when all was ended suddenly by his untimely death.

Professor Johnson, on May 3d, 1892, became a Member of the Institution of Civil Engineers, of Great Britain. On March 14th, 1899, he was elected a Member of the Western Society of Civil Engineers, and thereafter until his death took a very active part in its work. He was also a member of the American Society of Mechanical Engineers; the International Association for Testing Materials; the American Association for the Advancement of Science; the Academy of Science, of St. Louis; the Art Club of Madison, and the University Co-operative Association. He was also a very earnest and active member of the Unitarian Church, of Madison, as he had formerly been of the Church of the Unity (Unitarian), at St. Louis.

In 1878, the year of his graduation from the University of Michigan, he was married to Miss Phebe Henby, who, with three daughters and two sons, survives him.

The large work accomplished by Professor Johnson is proof of the untiring industry and devotion to duty which always characterized him. What his hand found to do, he did with his might.

As a teacher he had few equals. He not only knew his subject himself, but he knew how to impart his knowledge and to help his pupils so that they should thereafter need no help.

Outside of his special work he was ever mindful of his duties to the city, to the State and the world. He was a good citizen, a valiant fighter against ignorance and evil. He was faithful to every duty, whether private or public; a man whose death is a loss to all men.

Professor Johnson was elected a Member of the American Society of Civil Engineers on April 7th, 1886. In 1899 he presented a paper* entitled "Cast Iron—Strength, Resilience, Tests, and Specifications," and was a frequent contributor to the discussions in the *Transactions* of the Society.

Transactions, Am. Soc. C. E., Vol. XXII, p. 91.

WILLIAM SHATTUCK LINCOLN, M. Am. Soc. C. E.*

DIED MAY 16TH, 1902.

William Shattuck Lincoln was born on May 19th, 1837, at Dennysville, near Eastport, Maine. He was the son of Theodore and Elizabeth (Lincoln) Lincoln. One of his great-grandfathers was Major-General Benjamin Lincoln, a distinguished soldier of the Revolution. Of his early life and schooling we have very scanty information. There is, however, evidence pointing to a residence for a time in Boston or, possibly, in Cambridge, though his name is not found on the Harvard rolls. But, however this may be, in his active and well-trained mind he had the best results that any school can give.

At the age of nineteen, following an older brother, he went West, and in October, 1856, we find him a Division Engineer, in charge of construction of the Cincinnati and Chicago Railroad, from Richmond to Logansport, Indiana. Here he remained until January, 1861, when he was appointed Principal Assistant Engineer in charge of construction of the Chicago and Cincinnati Railroad, between Logansport and Valparaiso, Indiana. Both these lines now form parts of the Pennsylvania Railroad line between Richmond and Chicago.

In July, 1865, he entered the service of what is now the Wabash Railroad Company, in which service he remained until his death—a period of nearly thirty-seven years. His first position in this service was that of Resident Engineer of the Western or Illinois Division of the Toledo, Wabash and Western Railway, with headquarters at Springfield, Illinois.

In June, 1868, he was appointed Chief Engineer of the Decatur and East St. Louis Railroad, built between these points as part of the Wabash System. Upon the completion of this line, in January, 1871, he resumed his position as Resident Engineer of the Western Division, a position which he retained for nine years. During this time he also acted as Consulting Engineer of the Lafayette, Bloomington and Mississippi, and of the Lafayette, Muncie and Bloomington, Railroads.

In January, 1880, after the Toledo, Wabash and Western Railway and the St. Louis, Kansas City and Northern Railway had been consolidated as the Wabash, St. Louis and Pacific Railway, Mr. Lincoln was appointed Engineer for the lines west of the Mississippi River. In September, 1881, he was appointed Chief Engineer of the whole system, with headquarters at St. Louis—a position which he held until his death.

In 1872 Mr. Lincoln married Lula, daughter of Captain Alvin M. Higgins, of Logansport, Indiana. This congenial and happy marriage

* Memoir prepared by Robert Moore, Past-President, Am. Soc. C. E.

was ended on May 4th, 1894, by the death of Mrs. Lincoln at their home in Logansport.

His own death occurred at St. Louis, on May 16th, 1902, at the Mullanphy Hospital, where, a few days before, he had undergone a serious surgical operation in the hope of obtaining relief from a severe neuralgic affection from which for years he had been an almost constant sufferer. He was buried by the side of his wife at Logansport, Indiana, leaving no children to survive him.

Mr. Lincoln was elected a Member of the American Society of Civil Engineers on December 5th, 1883. As an engineer, he was careful, thorough, conscientious and sound. The entire confidence reposed in him as an engineer and as a man, by those having the best possible means of judging him correctly, is shown abundantly by his long-continued service with one railroad system, through many vicissitudes of management, reorganization and control. But, complimentary as was this long service with one corporation, it had the effect, when coupled with his retiring and home-loving disposition, of limiting the range of his acquaintance as well as the scope of his professional work; so that he was not as widely known as his great merits would otherwise have made him. By those, however, who had the opportunity of knowing him he was held in the highest esteem. Unassuming and unselfish, clear-headed and strong, with a cheerful fortitude which years of suffering could not shake, he was a man of noble type whom it was a privilege to count as a friend, and whose memory is a lasting possession to all who knew him.

GEORGE HENRY MENDELL, M. Am. Soc. C. E.*

DIED OCTOBER 19TH, 1902.

George Henry Mendell was the eldest of four children—a girl and three boys—all of whom he outlived.

He was born on October 12th, 1831, in the Village of Youngstown, Westmoreland County, Pennsylvania, from which place his parents moved to Blairsville, in the adjoining County of Indiana, in 1837.

His father, George S. Mendell, was a teacher, and evidently gave his son a very thorough grounding in English, Latin, Greek, and the rudiments of mathematics, which was followed by the instruction given in the Blairsville Academy, by R. McCall, a graduate of Jefferson College. The course in the Academy embraced Historia Sacra, Cæsar's Commentaries, Virgil's Bucolics, Georgics and Æneid, Horace, Cicero's de Officiis, de Senectute and Amicitia, etc., in Latin, and the Anabasis and the Iliad, in Greek, with some mathematics. This course was completed in the years 1843–47 in very creditable style.

During two years of this time he was also an instructor, and received his own instruction free.

In the autumn of 1847, at the age of 16, young Mendell undertook to teach school for four months, which duty he performed very creditably for the modest pay of $20 per month.

In 1847–48, during a visit to relatives in Massachusetts (the native State of his parents), he heard of West Point Military Academy, and, through his own exertions, obtained the appointment from Congressman Buffington, entered on July 1st, 1848, and was graduated third in his class in 1852. He selected the Topographical Engineer Corps as his line of service, which corps, during the war, was consolidated with the Engineer Corps, in which he remained until he was retired at the age of 64 in 1895.

He was Assistant Topographical Engineer on the survey of the Northwestern Lakes, in 1852–54, then on the staff of Major-General Wool, commanding the Department of the Pacific in 1854, at which time the advantages of the Pacific Slope made a lasting impression on his mind.

He was thereafter assistant on a railway exploration survey from San Francisco to Fort Yuma, and later, in 1855 and 1856, on an expedition against hostile tribes in Oregon and Washington Territories. From 1856 to 1858, he was in charge of the construction of military roads in Oregon and Washington Territories. He was then called to the Military Academy at West Point, and served there as Assistant

*Memoir prepared by Messrs. C. E. Grunsky, L. J. Le Conte and Marsden Manson, Members, Am. Soc. C. E.

and Principal Assistant Professor of Natural and Experimental Philosophy.

At the outbreak of the War of the Rebellion he was assigned to Colonel Miles' Division in the Manassas Campaign, and thereafter rendered valuable service on special duty and in command of the United States Engineers' Battalion in reconnoissance work, building, guarding and destroying bridges, constructing batteries, blockhouses, rifle-trenches, making and repairing roads, and in carrying on siege operations at Petersburg, Virginia. He was Assistant Engineer on the defenses of Baltimore, Maryland, in 1864. After again serving as instructor in the Military Academy for a year, to which duty he had been assigned to recover from disabilities incurred in the field, he was, for a brief period, Superintending Engineer of harbor defense works at New Bedford, Massachusetts, and of the work for the preservation of Plymouth Beach.

In 1867 he was transferred to California, and took charge of the fortification work of Alcatraz Island and Lime Point, in San Francisco Harbor, and, soon after, the defenses at the mouth of Columbia River and of Fort Point, San Francisco.

His activity was not confined to his Government work, which included the Wilmington Harbor breakwater, examination and survey of Estero Bay and San Diego Harbor, the removal of Rincon Rock in San Francisco Bay, the improvement of Oakland Harbor and of the Sacramento and Feather Rivers and of the construction of San Francisco Harbor defenses, for he was a member of the Commission appointed in 1874 to examine the irrigation possibilities in California. He was a member and President of the first California Débris Commission to regulate hydraulic mining. He was on the Commission to select a site for a naval dry dock on the Pacific Coast north of Latitude 42°, and on the Commission to select a site for a military post in San Diego, California, and of the Board of Engineers for the planning of Pacific Coast defenses.

Colonel Mendell was consulted by the City and County of San Francisco in reference to the sufficiency of the City Hall concrete foundation. In 1876-77 he was entrusted by that city with the examination of possible sources of water supply, and made an exhaustive report, which has proved of great value to the city. From 1878 to 1880 he was Consulting Engineer to the State of California, acting as an adviser to the State Engineer, who was charged with the investigation of irrigation, drainage and mining débris problems. He was also a member of the Sewerage Commission for the City and County of San Francisco, appointed in 1892 by the Board of Supervisors.

At the time of his retirement he was the Senior Colonel in the Engineer Corps of the United States Army.

Among the works with which he was more or less prominently con-

nected as Consulting Engineer subsequent to his retirement may be mentioned the Portland Water-Works, the appraisement of the Los Angeles Water-Works, the examination of the water supply possibilities for Berkeley, the drainage project for Reclamation District, No. 108, in the Sacramento Valley, and the Red Bluff Water-Works.

He was selected by Mayor James D. Phelan to serve under a new city charter as President of the first Board of Public Works for the City and County of San Francisco, from January 8th, 1900, for three years. His efficient service in organizing this new department, which benefited by his long experience, his thoroughness as an engineer, and his ripe judgment, are thoroughly appreciated by those who have been in a position to judge of the results. His term of office was drawing to a close at the time of his death.

Colonel Mendell was one of the founders of The Technical Society of the Pacific Coast, and was made its first President in 1884, serving in this capacity for two terms.

General William P. Craighill, Past-President, Am. Soc. C. E., a life-long friend and fellow officer in the Corps of Engineers, writing of Colonel Mendell, says:

"I have known him since 1849, when I went to West Point, and our acquaintance began with my first appearance as a 'plebe' in the squad he supervised as drill-master. He was then, as he always was, a man who did his duty well and thoroughly; but there was a gentle way in his manner, tone and expression, which proclaimed him, as he always was in every relation of life, a gentleman, the highest type of man.

"I do not know that I can say more in praise of my dear friend than that, after an intimate acquaintance of more than fifty-three years I have always found him to be a real Christian gentleman, and I believe this will be the verdict of everyone who knew him. His mental ability none can deny. It was great; far beyond the average."

L. J. Le Conte, M. Am. Soc. C. E., who was his chief assistant for twenty-one years, has this to say:

"I have served under Col. Mendell, both on public as well as on private works, and have uniformly felt the genial warmth of his friendship and the sound wisdom of his counsels. He was eminently fitted to supervise, and did so with the fullest regard to the preconceived opinions of those beneath him, and yet withal used such consummate tact as to make everyone feel an everlasting regard for him and his conclusions. Few men were gifted in this regard as he, and still fewer could smooth over seemingly insurmountable differences, and bring everything down to one harmonious conclusion. His loss will be greatly felt by the entire community, which he so faithfully represented. Peace to his ashes."

In 1858 he was married to Ellen Adair, daughter of the late John Adair, Esq., Collector of Customs at Astoria, Oregon, and grand-daughter of General John Adair, Governor of Kentucky, Senator and Representative in Congress, a revolutionary soldier who re-entered the service of his country in the war with England in 1812, and com-

manded the Kentucky troops at the battle of New Orleans. This union of two congenial spirits proved a happy one. Through forty-four years they lived in affection and love, devoted to each other and to their children. At the end of that time, while resting on her arm and being administered to by her loving hand, Colonel Mendell, with his mind clear and his eyes looking in her anxious face, closed them to all earthly things, while his pure spirit took its flight to the better realms, there to await the coming of hers.

A just estimate of Colonel Mendell's worth and appreciation of his professional work is fittingly expressed in the words of the Chief of the Engineer Corps of the United States Army, who, in sending out notice of his death, says:

"Colonel Mendell's record for distinguished service, his high attainments, his purity of life, and his sincerity of purpose, in all matters relating to either private or public official work, have never been excelled by any officer whose record has appeared upon the rolls of the Army."

His entire life, from boyhood to the ripe age of seventy-one years, was crowded with active responsibilities far beyond those which fall to the average man. All these he met and discharged with the highest ability and integrity. In these crowded duties, he still found room for the exercise of a benevolence rarely equalled in its usefulness. He was one of the founders of St. Luke's Hospital, in San Francisco, and for many years one of its Directors, and never ceased to take an interest in the welfare of that institution.

Gentle, noble, and great in the discharge of every duty, he left kind memories with all who knew him.

Col. Mendell was elected a Member of the American Society of Civil Engineers on September 6th, 1876, and served as its Vice-President from January 20th, 1897, to January 18th, 1899.

MARTINIUS STIXRUD, M. Am. Soc. C. E.*

DIED DECEMBER 28TH, 1901.

Martinius Stixrud was born in Thoten, Southeastern Norway, on January 28th, 1857. He was graduated from Chalmers Technical Institute, at Gothenburg, Sweden, in 1878, and continued the study of Civil Engineering at the Polytechnicum, at Aachen, Germany, during the next year. In the spring of 1881 he came to the United States.

His first summer in this country was spent as a transitman and draftsman on the Manitoba Railways. During the winter of 1881-82 he was engaged as draftsman and computer in the Bridge Department of the Chicago, Milwaukee and St. Paul Railway, at Minneapolis. Early in 1883 he was engaged by the Northern Pacific Railroad, with office at Brainard, but from there was sent out to the Pacific Coast by that company.

In 1883 he designed the switch-back over the Stampede Pass, under V. G. Bogue, M. Am. Soc. C. E., who at the time was Principal Assistant Chief Engineer of the road. Mr. Stixrud continued his engagement with this railroad until 1885, and during part of this time he was Assistant Engineer to W. H. Kennedy, M. Am. Soc. C. E. In 1885-86 he was Engineer on the location and construction of the Oregon Pacific Railroad. During 1886-87 he was Locating Engineer for the Seattle, Lake Shore and Eastern Railway, running lines across the Cascade Mountains through the Snoqualmie Pass. This party suffered a great deal during the winter, from exposure and shortage of provisions, and at that time Mr. Stixrud contracted an ear trouble which caused him much pain ever afterward.

In 1887-88 he was Engineer in Charge of Location and Construction of the Seattle, West Coast Railway (afterward the Seattle, Lake Shore and Eastern Railway). In 1888 he went to Spokane, in the interests of the same company, and designed and constructed the bridges, of this company, over the Spokane River. In 1889 he continued as Locating Engineer for the same company. In short, up to this time he had been engaged constantly on railroad work, on both location and construction, and his reputation as a reliable and able engineer in those branches was established.

In 1890, very much against his inclination, Mr. Stixrud became City Engineer of Seattle, but did not give satisfaction to the politicians, who were unable to use him and his office as they pleased. He was most shamefully treated, and although ousted from office, was completely exonerated, and came through this blackmailing process

*Memoir prepared by S. T. M. B. Kielland, M. Am. Soc. C. E.

victorious. Some time before this, he was engaged with J. E. Ericson, M. Am. Soc. C. E., later City Engineer of Chicago, in preparing plans for water and sewerage systems for Seattle, under Mr. Benezette Williams.

From 1890 Mr. Stixrud was located permanently in Seattle, and practiced as a Consulting Engineer, forming a partnership in 1892 with Mr. C. Näsfen. To be a consulting engineer in that western country required the most diverse knowledge and practice, thus bridge and structural engineering, water supply, sewerage, drainage, irrigation, and also railroad surveys, comprised the firm's work.

In 1892-93 Mr. Stixrud spent the winter in California and Mexico, having in view the irrigation of the desert lands in the Colorado River Basin. Plans were prepared to irrigate 600 000 acres. The proposed intake was on the Colorado River near Hanlon's Ferry, not far from Yuma. However, the business part of the undertaking failed at that time. In 1893 he was engaged as Engineer for the Board of Tideland Appraisers for King County, Washington. In this capacity he made a very extensive survey of Seattle and Ballard Harbors, establishing harbor lines and waterways, and plotted the tideland areas at Seattle, Ballard and part of Tacoma Harbors. Especially for Seattle, this was a work of great importance, as it dealt with the future plans of Seattle Harbor, railway terminals and manufacturing districts. Mr. Stixrud did not succeed in getting his general plan of the main part of Seattle Harbor accepted. Captain T. W. Symons, M. Am. Soc. C. E., representing strong interests, had a revised plan, which was accepted. Mr. Stixrud's plan was one with tidal basins, the rise of the tide being 16 ft. Captain Symons' plan was for open waterways, which appeared to suit the immediate or near future. Mr. Stixrud had ignored the South Canal Waterway.

In the winter of 1896-97 Mr. Stixrud was the Chief Engineer for one of the cable tramways, constructed over the Chilkoot Pass, Alaska.

Mr. Stixrud died in Seattle, after an illness of seven weeks, the cause of death being congestion of the brain, considered to have been caused primarily by his exposures in the Snoqualmie Pass in the winter of 1886-87.

The foregoing gives a general outline of Mr. Stixrud's professional work. As to his character, too much cannot be said. He was of a cheerful, pleasant disposition; admired and respected by all, and was an excellent representative of the Scandinavian race—honest, true and brave. In appearance he was tall, erect and fair. He left no fortune, but many friends. He was not married.

Mr. Stixrud had many qualities through which he was especially adapted to his profession; he was of an inventive mind and had great designing abilities, mechanical devices being his hobby. Where he

made his home he was an engineer of high standing, but, without doubt, he would have attained more prominence if he had been favored by a location in which his pure engineering qualities would have been appreciated more than could have been the case in the new and undeveloped West. F. W. D. Holbrook, M. Am. Soc. C. E., under whom Mr. Stixrud served as an assistant during the construction of the Seattle, Lake Shore and Eastern Railway, in 1888-90, writes as follows:

"Mr. Stixrud was a young man of fine physique and presence, and had been well educated in foreign technical schools. He was very companionable, and of sterling integrity. I mention this last, as after having taken the office at one time as City Engineer of Seattle, when his political opponents trumped up some charge against him that led to his resignation, but no one knowing the facts ever laid anything to his discredit on that account."

W. H. Kennedy, M. Am. Soc. C. E., Chief Engineer of the Oregon Railroad and Navigation Company, and under whom Mr. Stixrud worked for several years, on the location and construction of the Northern Pacific Railroad, writes:

"Shortly after Mr. Stixrud's arrival in this country, he was engaged with me, on the location of the Green River Division of the Northern Pacific Railroad, as topographer. I can only say, that he was one of the finest topographical engineers I have ever known, and a location based on his topography rarely required to be amended after being once run in on the ground. I knew him intimately from that time until his death, and had no friend whom I esteemed more highly. His loss at such an early age was a loss to the profession, of which he was an honored member. I had always looked forward to a brilliant career for him, as he had marked ability as a designer and has left many monuments of his skill as such in Seattle, Washington, where, at the time of his death, he was in the active practice of his profession."

The foregoing will give some idea of the life and character of Martinius Stixrud, but the ennobling effect of his company, the kind but manly and strong appearance of the man, his excellent manners, and his sincere friendship toward those who had the privilege of being near him, can only be appreciated fully by those who came in contact with him. He was sincere and sober in business, but in conversation and discussion he was bright and entertaining. Mr. Stixrud was one of the most enthusiastic yachtsmen of Seattle, and his boat was for a long time the champion of her class.

Thus his life was vigorous, strenuous and manly. His noble example as a man and engineer will have a lasting and elevating effect on all who knew him.

Mr. Stixrud was elected a Member of the American Society of Civil Engineers on April 3d, 1889.

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